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DEVOTED TO THE ADVANCEMENT OF ASTRONOMY AND ALLIED SCIENCES

EDITOR, C. A. CHANT ASSISTANT EDITOR, RUTH J. NORTHCOTT

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THE SOCIETY'S NEW HOME

EARLY this year a milestone in the history of the Royal Astronomical Society of Canada was reached with the purchase of a permanent home.

As early as 1910, when a building fund was first set up, the Society had recognized the desirability of eventually owning a building to house its office and library. Of recent years as conditions forced frequent changes to less and less adequate rented quarters at greater and greater expense, the need for permanent headquarters had become acute. Finally, last year, a committee was appointed to take the required action to purchase a suitable building.

In January of this year, acting on the advice of the Committee, the Society purchased a building which appeared to be in every way suitable. The building (see photograph) is located on a 40-foot lot on the north side of College Street between Huron Street and Spadina Avenue. Until recently it had belonged to the late Dr. Isabella Wood who, some years ago, had remodelled the house to provide three small apartments on the upper floors as well as her own residence and medical office on the ground floor. This arrangement enables the Society to obtain revenue from rentals to help meet operating expenses.

The purchase price of the property was $32,500. An initial payment of $12,500 was made from the Society's building fund and other reserves, and a mortgage was arranged for the remaining $20,000. Approximately $1000 was allotted for the relatively few alterations which were needed and for redecorating and building cupboards and library shelves. A further $1000 or more will be needed for new furniture.

As part of an effort to reduce the mortgage and to ensure financial success for the new venture, the Society has appealed to its members to make contributions to the building fund, and an objective of $12,000

The new home of the Society, 252 College Street, Toronto 2B.

was set for this appeal. Already more than half of this amount has been received or pledged. In addition there have been some fine gifts of services. Members who have not yet contributed are invited to send contributions to Mr. J. H. Horning, National Treasurer, Royal Astronomical Society of Canada, Oakwood Collegiate Institute, Toronto. A receipt valid for Canadian Income Tax exemption will be mailed promptly.

The new address of the Society is 252 College Street, Toronto 2B. All members and friends of the Society are cordially invited to pay a visit and to use the reading room and library.

J. F. H.

FORTY MINUTES WITH EINSTEIN

By A. Vibert Douglas

In early January 1954 I visited Dr. Albert Einstein as a direct consequence of a research into the history of cosmological thought during the years immediately following the publication in 1915 of his General Theory of Relativity. I wanted to weigh the contributions of the three immediate contributors to the development of this theory, namely de Sitter, Weyl and Eddington, and especially to see Eddington's contribution through Einstein's eyes.

I walked from the Princeton railway station to the Institute for Advanced Studies and was told that Dr. Einstein had a severe cold but would see me at his home. The Institute bus took me to the white wooden house on a quiet street of Princeton and the housekeeper showed me into the parlor. My eye was immediately caught by two inlaid cabinets containing various objects of religious art. On a canopied central shelf of one was a rather beautiful Madonna and Child; on a side table was a statuette of a Chinese philosopher beggarman; on the wall hung a picture of the period of early Italian Christian painting.

Upstairs at the back of the house was his study. The rear wall was almost entirely of glass to the floor and looked out over back gardens and fields beyond, with leafless trees making filigree patterns against the grey-blue winter sky. Along the opposite wall and one side wall were high bookshelves, a few of the shelves holding piles of offprints.

Dr. Einstein was seated in an easy chair at the angle of the bookcases, a low square table in front of him, and his secretary with notebook on knee by the table on his right. His massive head, longish white hair, powerful very dark eyes under shaggy grey-white eyebrows, caught and held attention. He wore a dark grey dressing gown and had a brown silk muffler round his throat. In spite of his obvious frailty, he insisted on rising to greet me.

He came directly to the point of my visit and paid a striking tribute to the English astronomer, Sir Arthur Eddington, who was the first and greatest interpreter of the theory of general relativity to the English-speaking world. He spoke of the literary value, the beauty and brilliance of Eddington's writing in those books aimed at giving to the intelligent lay reader at least some understanding, some insight into the significance of the new scientific ideas—but with a smile he added that a scientist is mistaken if he thinks he is making the layman understand; a scientist should not attempt to popularize his theories, if he does "he is a fakir—

it is the duty of a scientist to remain obscure". I said I could not agree, that the scientist had a duty to try to educate the public at least to an appreciation of what the scientist is attempting to do; but Dr. Einstein shook his head.

Of Eddington's great book *The Mathematical Theory of Relativity* he had only the highest praise believing that it still stands as the finest presentation of the subject in any language. When asked what he considered Eddington's greatest contribution to relativity theory apart from his expositions of it, Einstein said, "He was one of the first to recognize that the displacement field was the most fundamental concept of general relativity theory, for this concept allowed us to do without the inertial system."

In regard to the developments in the early years made by Weyl and Eddington, the later theories of the expanding universe of Friedmann, Lemaître and Eddington, the still later kinematic relativity of E. A. Milne, and the yet more recent theories of continuous creation of matter of Jordan, Bondi and Hoyle, the comments of Dr. Einstein were brief and critical. He definitely disliked the hypothesis of continuous creation, he felt the necessity for a "beginning"; he regarded Milne's brilliant mathematical mind as lacking in critical judgment; he was not attracted by the idea of Lemaître's primeval atom; and he concluded by saying of his own and all the others, "Every man has his own cosmology and who can say that his own theory is right!"

I asked him why so many Americans were unwilling to accept the idea of the expansion of the universe and the observed red shifts of the spectrum lines of distant galaxies of stars as genuine Doppler effect of recessional velocity. His reply was very brief: "They don't understand general relativity!"

Reverting to the theory of continuous creation and the controversy in London over the legitimacy of a theory not based on observable facts, Dr. Einstein approved the warning which had been sounded by the President of the Royal Astronomical Society the previous year, but he added that we must not forget that Clerk Maxwell's fruitful theory was not laid upon a foundation of physical observation.

He then told me of his visit to Cambridge to receive an honorary degree from the Chancellor of the University in 1930, when he was the guest of Professor and Miss Eddington at the Observatory. He spoke of the quiet hospitality of their home and recalled the fact that during the time of this visit the King's Birthday Honours appeared in *The Times* with Eddington's name in the list of Knighthoods. I asked whether Eddington showed any reluctance about accepting a title and Dr. Einstein

replied abruptly, "No, why should he?" I said that some members of the Society of Friends strongly disliked such marks of approval and I had wondered if Eddington had had any hesitancy in accepting this royal honour. He said not only was Eddington gratified but his sister was obviously very happy about this honour to her brother. Dr. Einstein then paid a sincere tribute to the Society of Friends and said quietly, "If I were not a Jew, I would be a Quaker."

From Eddington's knighthood my thoughts jumped to the New Year Honours just published two days previously in London and I mentioned that Epstein would now be Sir Jacob. I thought Dr. Einstein would be specially pleased, but he seemed quite indifferent. Laughingly I said that the British had compiled quite an assortment of their citizens: a sculptor, a comedian, a track champion, the organizer of international football, and the usual array of scientists, civil servants and leaders in various phases of active citizenship at home and in far parts of the Commonwealth. "Yes," he replied, "it is quite a zoological garden—lions, elephants *and* giraffes".

On the low table between us were pages of mathematical analysis, ash trays and a beautiful little carved wooden monk. I remarked upon his choice objects of religious art asking if he had any special interest in Roman Catholicism. He shook his head emphatically and said that solely as art he enjoyed these things.

Thirty years ago, I had been told that in his study in Berlin were three portraits, one was of Sir Isaac Newton to whose universal law of gravitation Einstein had made the first modification, the next was of James Clerk Maxwell, and the third was another picture of Newton. I looked to see if Newton's picture was on the wall of his study in Princeton. Instead I saw a portrait of Mahatma Gandhi and another striking portrait which Dr. Einstein told me was of a German musician whom he held in high esteem. Of Gandhi he said, "He was the greatest man of our age". I thought of Dr. Schweitzer and mentioned his name, evoking the remark, "Yes, he too is a very great man".

I mentioned a scientist whose work I had reason to admire, a man who had once worked closely with Einstein and who, a few years ago had returned to academic work in one of the countries now under Soviet domination. Did he ever hear from this man?—Yes, he had heard from him about some mathematical matters, but "he does not say the things which I imagine he would like to say; he made a mistake in going back, he should have known . . . I am no hater of Russia but I would not like to work under the intellectual restrictions they have created".

My time with Dr. Einstein was drawing to a close and there remained

one special thing which I wanted to ask him—Who were the greatest men, the most powerful thinkers whom he had known? The answer came without hesitation, "Lorentz". Did he place any others on the same level as this great Dutch mathematical physicist of Leiden?—and I named several of the famous theoretical physicists and cosmologists. No, this one was too uncritical, that one was uneven, another was of a lesser stature . . . but, he added, "I never met Willard Gibbs; perhaps, had I done so, I might have placed him beside Lorentz". On one of the deep bookshelves stood a small leather-framed snapshot of Professor H. A. Lorentz.

One other name was in my thoughts, the mathematician, Minkowski. Where would Dr. Einstein place him? The answer was unexpected: "He was my very great teacher in Zurich, but I am not a good enough mathematician to know where to place him."

As I walked towards the railway station I thought over much of what had been said and realized that many of Dr. Einstein's remarks were impressed verbatim on my mind. I recalled what he had written in his *Self-Portrait*, "I live in that solitude which is painful in youth, but delicious in the years of maturity". Yet he had given to a complete stranger forty minutes of his 'delicious solitude', conversing with a serenity, wisdom, humour and quiet friendliness which are unforgettable. He was a truth-seeker in the deepest profundities of truth, and truth-seeking was itself an unquestioned highest goal. He was one of the supergiants of the human race.

Queen's University,
Kingston, Ontario.

THE COLUMBUS MEETING OF THE AMERICAN ASTRONOMICAL SOCIETY

By K. O. Wright

The ninety-fourth meeting of the American Astronomical Society held at Columbus, Ohio, from March 21–24, 1956 was most successful. The Ohio State University acted as host on this occasion and the regular meetings of the Society were held in the large and pleasant auditorium of the Ohio State museum. A reception by President Howard L. Bevis was held in the President's residence on the evening of the first day, while a second reception was held at the Perkins Observatory on Thursday evening by President Frank J. Prout of the Ohio Wesleyan University. At luncheon on Friday, the Teachers' Committee of the Society had invited Dr. Raymond J. Seeger of the National Science Foundation to give a talk on "The Young Astronomer". There has been a feeling that the teaching of astronomy has not been given the prominence that it deserves, especially in view of the apparent need for stressing the value of the sciences as a profession to the best high school graduates. The special address at the Society dinner on Friday evening was "The Beginnings of Radio Astronomy" by C. M. Jansky, brother of Karl Jansky who, in 1932, was the first to detect extra-terrestrial radio waves. Karl Janksy died in 1950 at the age of 44 without knowing that the field of radio astronomy would very soon become one of the most active in the science.

A symposium on "Radio Telescopes, Present and Future" under the chairmanship of Dr. Merle Tuve of the Carnegie Institution of Washington, was held on Friday afternoon. In discussing "The National Radio Observatory", Dr. Berkner of Associated Universities noted that there is no substitute for a large radius in building an antenna, to achieve resolution, speed and sensitivity. Plans are now underway to build a first national radio-telescope with a 140-foot antenna with $f = 0.5$ at Greenbank, W. Va., where the noise level is very low, and there are hopes that a national radio park may be established there. Researches at the Carnegie Institution, California Institute of Technology and Ohio State University were also described.

At the regular sessions of the Society, some seventy papers covering the most active fields of research in astronomy were presented. The radio noise coming from Jupiter, discovered last year by Franklin and Burke, has been studied intensively at several observatories. The noise has been shown to come from localized regions on the surface of the planet, some of which are near the well-known red spot.

In a paper on "The Dead Stars of Population I", Deutsch discussed

stars with lifetimes less than that of the galaxy and concluded that such stars may not be white dwarfs, which are usually of population II, but could be stars such as alpha Herculis which shows displaced spectral lines indicating that it is losing mass at the rate of one solar mass in ten million years. Thus five hundred generations of red giants could give the required number of "dead stars", and M-type giants may be the final stage of massive, population I, giant stars.

From a study of three Mount Wilson high-dispersion spectra of Algol taken in the red region during eclipse, Meltzer has found lines due to a third body with a spectrum about F5. These data agree with Eggen's photometric observations, and, from a study of the radial velocities, it was found that the mass of the third body is nearly the same as the sun and one-quarter the combined mass of the two stars that provide the eclipses.

McLaughlin has recently suggested that changes on the surface of Mars may be due to volcanoes. He has examined many photographs of Mars in the region of L. Phoenicis and has found several examples of clouds forming over this small area (see Whipple, *Earth, Moon and Planets*, p. 217) or of changes taking place, between 1877 and 1954. McLaughlin attributes the frequent occurrence of clouds in this small area to volcanic action.

Hynek discussed problems of tracking the proposed artificial satellite that is to be launched during the International Geophysical Year, 1957–58. He said that twelve 20″, f/1 Schmidt telescopes are being built to photograph the satellite as it moves very rapidly across the sky. Photographs will be made on 55-mm. film in a cycle of 1, 3 or 5 seconds and timing will be accurate to 1/1000 second. During the beginning and near the end of the life of each satellite, all possible observations will be required and amateur aid is to be enlisted; however, according to present plans, it is unlikely that the satellites will be visible from any part of Canada.

Canadian representation at the Columbus meeting was quite large, with three representatives from Toronto, three from Ottawa and one each from Western Ontario, the National Research Council and Victoria. Of the seventy-three papers on the programme, twelve were presented by Canadians or by members who formerly lived in Canada. As is usual, much of the value of the meeting came from informal discussions with other astronomers to compare observations, to draw up future plans and to discuss problems of mutual interest. The astronomical staffs of the Ohio State and Ohio Wesleyan Universities are to be congratulated and thanked for their part in making the meetings so worthwhile and so enjoyable for all those who attended them.

THE FORMATION OF THE PLANETS, PART II

By Gerard P. Kuiper

Introduction. The problem before us is to account for the existence of planets, their orbits, masses, and gross compositions; their satellite systems, axial rotation, and obliquities; and, incidentally, the formation of asteroids and comets. Not included will be the subsequent, but still pre-geological, internal development of the planets, their detailed compositions, surface temperatures, heat balance, etc., subjects partly geochemical in nature.

The discussion will begin with a brief and necessarily incomplete review of the recent literature* and the development of the principal ideas now current; this part may assist in clarifying a subject on which opinions are still divided. Thereafter the present status of several subproblems is summarized.

A decade ago majority opinion was returning to the earlier picture of Kant and Laplace that the planets had formed within a flat, largely gaseous disk rotating around the sun. The kinematical properties of planetary motion (common direction of revolution, nearly circular orbits of low inclination) appeared compelling arguments for this. Such properties could not possibly have originated from interactions between a few large planets initially moving more or less at random. This argument must have been surmised already by Kant when he put forward his descriptive picture in 1755, just two centuries ago; and was apparently clearly seen by Laplace, when he published the first version of his nebular hypothesis in 1796. The importance of the argument was reiterated by several recent writers, following Poincaré (1913).

Actually, neither Kant's picture nor Laplace's hypothesis corresponds very closely to the modern version of the solar nebula. As Poincaré (1913) and others have pointed out, Kant violated conservation of angular momentum when describing the contraction of the postulated chaotic nebular mass, assumed to be without preferred direction of motion initially, but resulting in a rotating disk that gave the condensation products their kinematical properties later. Laplace's nebula, on the other hand, was a very extended solar atmosphere, upheld by the intense heat of the sun at that time, rotating with constant angular velocity throughout, and shedding rings of gas as the sun cooled and the nebula contracted. Each ring was assumed to have subsequently broken up into small clouds which later combined and made an extended planet—which

*A complete bibliography for each year is found in Sec. 55 of *Astronomischer Jahresbericht.*

then could go through the same process once more, on a smaller scale, and form satellites. In the later versions of the hypothesis, one ring was supposed to have remained broken and to have led to the four asteroids then known. Laplace rejected the possibility that condensations might take place inside the nebula because with the nebula having a constant angular velocity such bodies would have spiralled inward toward the sun. Laplace considered comets as condensations from interstellar space and the zodiacal light as a remnant of the solar nebula. For a fuller description of the Laplacian hypothesis and its amendments during the following century, reference is made to Poincaré (1913), who at the time of writing considered some modified Laplacian hypothesis still the most promising.

The Density and Momentum Difficulties of the Nebular Hypothesis. The question arises then why early in this century the Kant-Laplacian scheme was abandoned by most astronomers. There were apparently two main reasons: (1) if the mass of the present planets were spread out along Laplacian rings, these would not coalesce into planets, as was concluded by Maxwell in 1859 on the basis of a simplified model and by Moulton (1900) on more general grounds; (2) the sun has 0.999 of the total mass of the solar system but less than 0.02 of the angular momentum. This does not look like a possible end-product from the Laplacian development. One would at least expect that the sun would rotate with its maximum possible angular velocity compatible with stability, being some 200 times faster than what is found. This difficulty was first pointed out by Fouché in 1884 and was stressed by Moulton (1900). In addition there was grave doubt concerning the possibility of the nebula shedding Laplacian rings at all.

Particularly the momentum difficulty looked so formidable that, at the turn of the century, Moulton and Chamberlain* assumed that a stellar encounter had pulled out of a slowly rotating sun, a mass of sufficient magnitude and angular momentum to produce the planets. The momentum difficulty was thus avoided. They assumed further that the planets themselves formed by accretion of small condensations in the nebula, called "planetesimals". This appeared to account in a general way for the *composition* of the terrestrial planets and the meteorites. Two decades later stellar encounters were similarly invoked by Jeffreys and Jeans; but they assumed that the solar gases condensed directly into

*Apparently they were influenced by the appearance of the thousands of spiral nebulae being discovered just then. Interestingly enough, Laplace saw in the spiral nebulae confirmation for his nebular hypothesis and looked upon them as the normal process of stellar formation.

planets. Nölke (1930) showed that, instead, the ejected solar filament would disperse, it being very far inside Roche's limit appropriate to its low density. He likewise showed that the momentum difficulty had not been fully solved by the encounter hypothesis, a result also stressed by Russell (1935). It was clear, then, that all a stellar encounter could produce was a small solar nebula (Spitzer, 1939), of insufficient dimensions to produce the planetary system; furthermore, it was an extremely rare event in the entire galaxy.

The basis for the first difficulty of the nebular hypothesis, that of the small mass and density of the solar nebula, was gradually removed in the 1920's and 1930's by the development of astrophysics, with the resulting knowledge of solar, stellar, and interstellar abundances of the elements, all of which were remarkably similar. It became clear that composition of the planets was exceptional and that they must have formed of matter initially of roughly solar composition, which must have been correspondingly more massive. However, this concept developed at a time when the nebular hypothesis was in disfavour, largely because of the second (the momentum) difficulty (cf. Russell, 1935), so that it did not immediately affect the status of the nebular hypothesis. Von Weizsäcker (1944), when he returned to this hypothesis, made the correct inference that the mass of the solar nebula was about 0.1 sun.

The basis for the second difficulty, that of the angular-momentum distribution, has also been in the process of disappearing. The slow rotation of the sun is but one example of the general rule that main-sequence stars of solar type have very slow rotations (Struve, 1945), though on theoretical grounds one would expect that the initial rotations were, on the average, rapid. Apparently there is a common mechanism that slows down stellar rotation. It has often been supposed that this mechanism is ejection or evaporation; but as is readily shown, the mass losses so required are too large to be admissable. Alfvén (1942), on the basis of a theorem obtained by Ferraro in 1937, was the first to suppose that a star could lose angular momentum to an ionized cloud or a stellar envelope, provided a magnetic field were present. The explanation given by ter Haar (1950*a*) of the slow rotation of old stars is not valid; first, because a passing interstellar cloud, not connected physically with a star, will not affect the rotation of that star; and, second, because the angular-momentum transfer would in any case be zero in the model considered, the momentum vector being at right angles to both the magnetic lines of force and the direction of relative motion, and therefore directed to the star itself. The second objection might be remedied by a different treatment, allowing for the deformations of the lines of force, but the first objection remains. For an ionized cloud *attached* to the star, a stellar

magnetic field would cause the ionized part to rotate essentially with the star and the momentum loss could take place at a distance of several stellar radii, with a correspondingly slow stellar rotation resulting.

In applying this mechanism to the sun, one is led to assume that the ionized cloud was the inner part of the solar nebula itself; because, the shedding process by the contracting and rapidly rotating proto-sun, that caused the solar nebula, must have continued until the sun reached its ultimate dimension. The nebula was therefore in contact with the sun, as required, and the ionization of the innermost part will have steadily increased during the solar contraction because both the rotational sheer velocities and the solar radiation increased.

Now the exchange must obviously have taken place well inside proto-Mercury, or, say, *at most* at about 50 present solar radii. There the period of revolution is 41 days. It could not have taken place close to $R_\odot$ or the present period of rotation would have been much shorter than is observed. The present period of rotation gives the position of the *minimum* distance of the mean exchange surface, namely $35R_\odot$. The exchange could be due to a frictional force, brought about by viscosity; or to a mass loss. In the latter case the loss required would be quite small, less than one per cent. In either case, the mechanism should, if possible, explain at the same time the solar obliquity of 7°. This seems to require that the momentum loss was caused by motions that were symmetrical to the plane of the solar nebula. Then a loss of angular momentum to the nebula would, at the same time, have increased any initial obliquity of the sun. Since this was written, Dr. Lüst has informed me that he and Dr. Schlüter (1955) have examined the problem of momentum transfer by solar magnetic fields quantitatively in considerable detail.

A curious dilemma exists, that solar rotation was slowed down by a frictional brake applied to the solar equator; but that at present, nevertheless, the equatorial zone rotates faster than the rest of the solar envelope and that, moreover, this feature is considered to be a remnant of the formative period (Cowling, 1953). Actually, no contradiction need be involved. A disk rotating with the sun would naturally possess more angular momentum per unit mass than material at the solar equator and, upon collapse onto the sun, the disk material would rotate faster.

The Break-up of the Solar Nebula and the Formation of Proto-planets. Having dealt with the two basic obstacles that seemed to face any form of nebular hypothesis—and shown them to be only apparent—we have simplified the task of appraising the many conclusions concerning the different forms of this hypothesis published during the past three or four decades. Several of these conclusions will be reviewed; the order followed is not strictly chronological but in part by topic.

Babinet appears to have been the first to have made the explicit assumption, in 1861, that the angular momentum and total mass of the initial system were identical with present values. With Jeans we shall call this "Babinet's criterion". Babinet's criterion plus the Laplacian picture of shedding rings necessarily led to a model with a very strong central concentration of mass, as was shown by Roche; accordingly, it became generally assumed that the sun was essentially at its present dimensions when the nebula formed. For a discussion of this point, cf. Jeffreys (1929, pp. 12–13).

It is important to note for what follows that *a model based on Babinet's criterion cannot possibly explain the planetary system.* This is implicit in the discussion of the preceding section. The composition of the planets is so highly selective that at least the original mass must have greatly exceeded the present value. The assumption that the abnormal composition of the planetary system is due to previous differentiation in interstellar space is contrary to all information available on the physics of interstellar matter.

Poincaré (1913, p. 22) derived an important theorem on the stability (against dissipation) of a Laplacian ring, which figured prominently in subsequent studies by Jeans (1919) and Jeffreys (1929) and caused them to reject the nebular hypothesis (while Poincaré continued to favour it, although merely on general grounds). The basis for the rejection was that the density of a ring that would not dissipate by lateral expansion, would have to exceed 0.35 of the mean density of the slightly flattened ellipsoidal volume (fixed by Roche's model), containing the central mass. This stability condition is indeed not satisfied by the Laplacian model. However, as Berlage (1940, p. 535) has pointed out, Poincaré's result is based on the assumption that the ring rotated with constant angular velocity. If that assumption is dropped, a low-density ring will be stable, as is well known from the stability of the rings of Saturn. Furthermore, even if the assumption of constant angular velocity is retained, Poincaré's density criterion can be satisfied by a highly flattened disk in which self-gravity is important, a case which Jeans and Jeffreys did not consider. Such a disk must have a mass rather greater than that of the present planets though it need not be nearly as large as the sun; and the temperature (or rather the z-velocities) must be low. This is not to suggest that planets were formed from Laplacian rings, but merely to indicate that what appeared to be a rigorous proof against the nebular hypothesis in general, applied only to a special case. Jeans (1919, p. 274) in fact granted that liquefaction could save the stability of the Laplacian ring, but held that this would preclude subsequent formation of a satellite system. This difficulty does not arise with proto-planets (see below).

Jeffreys (1929, p. 13) also considered gravitational condensation within

the nebula, but ruled it out as contrary to the evolutionary sequence of Roche models considered by Jeans, all of which were symmetrical. However, Jeans considered constant angular velocity within each configuration. If, instead, the hydrodynamical equations are used, one finds that gravitational condensations can occur if the density is sufficient.

Berlage, convinced that some form of the nebular hypothesis was correct, studied in a series of papers starting in 1930 the hydrodynamical equations of the solar nebula. He reached several tentative conclusions on possible density distributions, taking into account viscous interaction. With the Babinet criterion the ultimate radial mass distribution did not correspond to the observed planetary masses, but rather to a roughly symmetrical distribution with respect to Jupiter (Berlage 1935, p. 862). However, the time scale for viscous interaction was probably longer than the life of the nebula. Berlage (1940, p. 534) avoids the momentum difficulty by assuming that the solar nebula was captured by a slowly rotating sun. He finds the nebular density too low for gravitational condensation, but concludes that rings can form within the nebula. These are supposed to break up and combine into planets, as in the Laplacian picture. In two later papers Berlage (1940) develops a perturbation theory showing that rings may form spontaneously within the nebula. He finds a satisfactory representation of the distances of the Jovian planets but not for the terrestrial planets, which he then supposes to have formed separately. He assumes that solid particles will form within each ring, which in time will combine into planets by a process that was not determined. In a later paper Berlage (1948) summarizes his work on the nebular hypothesis.

Alfvén (1942, 1954) has made a very different approach. He supposed that under the combined effects of solar gravity and a postulated magnetic field, low-density material fell into the sun and sorted itself out in concentric shells depending on the ionization potential. He concluded that four such shells would form, called *A*, *B*, *C*, and *D*, composed predominantly of helium, hydrogen, carbon, and (iron, silicon, plus magnesium), respectively; and comprising about 28, 71, 0.2 and 0.3 per cent. of the total mass (Alfvén, 1954, p. 15). The *B* cloud was assumed to form earth, Venus and Mercury, not from the hydrogen, but from impurities brought in by the infall of meteors (*op. cit.* p. 164); and the *C* cloud the Jovian planets. Mars and the moon (later captured by the earth) formed from impurities in the *A* cloud. A transfer of angular momentum from the sun to shells *B* and *C* was supposed to occur by a hydromagnetic action, leading to a flattening of the shells into rings parallel to the solar equator; and to a further subdivision of the two rings into several each, of the Laplacian type. Condensation thereupon led to planet formation, via droplets or particles.

The conclusions referred to were not derived from an astrophysical theory taking full account of the dynamical interactions and the physical properties of bulk matter, but were largely based on the expected behaviour of individual particles and tenuous streams. Some of the difficulties have been pointed out by ter Haar (1948*a*, *b*), whose comments were briefly discussed by Alfvén (1954, pp. 181–82). That large masses are involved follows from the result that ring *C* (forming the Jovian planets) contained 0.002 of the infalling mass. The total infalling mass must therefore have exceeded 0.5 sun, most of which (more than 0.35 sun) was contained in the ring producing the earth and Venus, and nearly the entire remainder in the adjacent ring producing Mars and the moon. Many *ad hoc* assumptions are introduced in the developments and the result that Jupiter and Saturn were formed in a ring composed largely of carbon and sulfur is contrary to the known composition of these planets, which is largely hydrogen. Furthermore, as is discussed below, the original mass of Jupiter was some 20 times greater than the present. This alone will rule out the hypothesis as it now stands. Since, however, stars embedded in emission nebulosity are known to exist, the programme of determining whether planets can form under such conditions is interesting.

The next study is by von Weizsäcker (1944). On the basis of planetary and cosmic composition, he estimated the mass of the solar nebula to be 0.1 sun, which was a definite step forward. He studied the density distribution of the nebula from an analysis based on the hydrodynamical equations, but neglected the self-gravity of the nebula. Thus he underestimated the density and overestimated the thickness by a factor which is about 24 for a nebula at the Roche density. The radial density distribution derived was smooth and gave no clue on planetary distances. He then postulated a geometric pattern of turbulence superimposed on the hydrodynamic model, as suggested by Bode's law of planetary distances; and assumed that accretion, eventually leading to planets, would progress most rapidly in the zones between the large eddies. The hydrodynamical aspects of this work were in several respects superseded by von Weizsäcker's general papers on cosmogony, but he did not return to the specific problem of planet formation. The subject of the 1948 paper was further developed by Lüst (1952) and von Hagenow (1955). Nölke (1948) showed that the eddies postulated in the 1944 paper could not arise from a system in laminar flow, while Chandrasekhar and ter Haar (1950) showed that they could not arise from a turbulent medium.

Schmidt published a series of papers, starting in 1944, of which Randič (1950) has given an informative summary in the English language. According to this account: "Meetings of astronomers and other Soviet scientists were held to discuss the new theory and its validity. . . Schmidt's

main idea is the possibility of the 'capture' of one celestial body by another. . . . He assumes that the sun, revolving round the galactic centre in a plane inclined to the galactic equator, has passed once through a cloud of interstellar matter and has captured a great amount of it. After capture this meteoric matter revolved around the sun in a lens-like concentration. It is possible of course that the particles revolved in opposite directions, but, since the density of the cloud is supposed not to be uniform, after some time the motion in one direction prevailed. Through collisions the particles lost their velocities and fell upon the sun or coagulated round the bigger particles forming eventually the planets. . . . To calculate the age of the earth Schmidt assumes that the meteorites revolving between the distances R = 0.8 and 1.3 astr. units fell eventually together and built the earth. . . . By integration and using the known values for the present radius and mass of the earth and of the estimated amount of meteorites falling on the earth in one year, Schmidt obtained 7.6×10^9 years. . . ." half of which was accumulated during the first 0.8×10^9 years when the cloud was not yet nearly exhausted. For further particulars, reference is made to the originals or to Randič (1950).

This work was followed by four papers by Gurevich and Lebedinsky (1950*a*, *b*), dealing, respectively, with "Formation of Planets", I, Gravitational condensation; II, Law of Planetary Distances and Planetary Rotation; III, Structure of Original Cloud and Division of Planets in Inner and Outer Group; and On the Properties of the Preplanetary Cloud. I recently acquired an English translation of the first of these papers, and Dr. A. N. Vyssotsky has kindly sent me a translation of the fourth. In the first paper the authors give a competent discussion of criteria for the occurrence of gravitational condensations and of the thickness of the nebula which takes into account the self-gravity. The physical picture differs from that considered in my studies, mentioned below, because the authors adopt the Babinet criterion and the sun at its present luminosity. They regard the origin of the solar nebula as uncertain. From their model they correctly conclude that gravitational condensation of the gas cannot take place, but assume that gravitational condensation will take place among the finely-divided condensation products. The disk formed by these particles will flatten, owing to inelastic collisions between the particles, until a critical density is reached. Then highly-flattened ellipsoidal clusters of particles will form which in time will probably coagulate, to masses that are found to be about 200 km. in diameter at the present distance of Jupiter and 1 km. near the present earth. It is not made clear what happens to the gas of the nebula, to what extent the cloud was of cosmic composition, and how this affects the Babinet criterion.

The main assumptions and conclusions of the fourth paper are quoted from Dr. Vyssotsky's translation. "At present there is no doubt that the planets originated from a cloud of diffuse matter which surrounded the sun. Since gravitational condensation is not possible for a gaseous cloud, it must have been a dust cloud. The unsolved problem is the origin of such a cloud. Let us assume that the cloud was initially gaseous and of stellar composition. We shall also assume that its angular momentum and mass were close to that of the present planetary system. As non-volatile elements condensed into a fine dust, the cloud became opaque. The surface temperature of the cloud, assumed spherical, was determined by radiative equilibrium." The authors thereupon compute the surface temperature of the gas sphere, neglecting internal absorption, and conclude that the sun would have heated the surface to a temperature at which hydrogen could just escape if the radius were similar to that of the present planetary system. They ascribe the radius of the system to this property. They further conclude that collisions between dust particles and friction with the gas would have caused the dust to form a flattened disk, while the gas cloud remained spherical. The dust cloud being opaque to solar radiation, it became very cold and in the zone of the Jovian planets even condensed some of the gas on the particles; this did not happen in the zone of the terrestrial planets. In this manner the density difference between the two groups is explained. Planetary formation from the dust followed the lines of the first paper, with the additional suggestion: "Other planets were formed under the influence of the already existing condensations of Jupiter. Owing to the tendency toward equipartition of energy, greater masses perturbed smaller masses, thus increasing their energies". This appears to be used as an argument for increased accretion.

It may be commented that Babinet's criterion appears to be used for the entire cloud, before its separation into two clouds, with the dust cloud acquiring most of the angular momentum. This does not seem possible. Since the dust had initially a total mass of less than 1 per cent. of the gas, the initial angular momentum was correspondingly small. Sedimentation will have slightly decreased this fraction, because dust, moving nearly in free Kepler orbits, will transfer angular momentum to the slower-rotating gas. Subsequent freezing of some of the gas to the dust cannot raise the fractional angular momentum above the fractional mass of the condensate. Also, the present compositions of Jupiter and Saturn as well as the much higher densities of Neptune and Uranus prove that condensation on dust is not the process by which these planetary masses originated.

Dr. O. Struve has called my attention to a book entitled "Voprosy Kos-

mogonii", (Moscow, 1952), of which the first three chapters have a bearing on this topic. They are: V. A. Nagnitzkii, "On the question of the density and compression of the outer layers of the earth"; V. A. Krat, "On the origin of the solar system"; and V. G. Fessenkov, "On the nature and the possible origin of meterorites, the zodiacal light and the asteroids". So far I have had no opportunity to secure translations.

Lebedinsky (1953), under the title "Recent Soviet Theories on the Origin of the Solar System" stated some general conclusions and commented on the importance of the work of O. J. Schmidt and his collaborators. In 1944 "he boldly rejected the two probabilities that were under discussion for a long time (being the nebular and encounter hypotheses), and assumed that the cloud was captured by the sun. . . . (The theory) takes into account in quantitative calculations the part played by the conversion of kinetic energy of the dust particles into heat energy and its subsequent emission into cosmic space. Of utmost importance are the deductions of Schmidt's theory pertaining to the history and structure of the Earth. . . ." The reader will wish to read this article in full; it is unfortunate that this one account by a Russian author in the English language is not more specific.

Edgeworth (1949), impressed by the momentum difficulty mentioned, concluded, as had Berlage and Schmidt, that the solar nebula was derived from interstellar material captured by the slowly rotating sun and was thus unrelated to the process of solar formation. He further concluded that condensation in the nebula would lead to the formation of small particles; that these would combine into small, gravitationally stable, clusters; that thousands or millions of these clusters would combine into planets; and that massive planets might retain some of the gas of the disk. Among the properties that were not explained are the magnitudes of the planetary masses; Bode's law; the fate of the remaining material of the nebula (presumably 99 per cent. in the zone of the terrestrial planets), planetary rotations and obliquities, and irregular (and perhaps regular) satellites. Gurevich and Lebedinsky (1950*a*) pointed out that Edgeworth's paper resembled that of an earlier study by Schmidt (1944).

Ter Haar (1950*b*), supplementing and revising his 1948*a* paper, studied the equilibrium and the time scale of turbulent dissipation of a solar nebula of adopted radial density distribution and having turbulence velocities of $1/\sqrt{3}$ times the orbital velocities (i.e. 17 km./sec. near the earth). He assumed the sun to be luminous as at present and neglected the self-attraction of the nebula. Apparently because of the excessive turbulence assumed, he came to the following unrealistic conclusions: the height at which the density of the nebula decreased by the factor 2

was 0.4 r; the dissipation took roughly 1000 years; and the satellites were formed before the planets acquired their gaseous envelopes. Ter Haar did not study the physical consequences of this highly supersonic turbulence on the temperature of the almost spherical cloud and establish that condensation could take place.

Kuiper (1949) posed the following problem: if it be assumed that the planets were formed in their present positions from proto-planets, which at the time of the break-up of the solar nebula essentially touched radially and thus divided the solar nebula between themselves, what are the numerical values of the masses required in order that these proto-planets be stable in the tidal field of the sun? It was found that "on the average, somewhat less than 1 per cent. of the mass of the proto-planets condensed into the planets themselves. This fraction is quite reasonable in view of the known planetary composition. . . . This percentage was undoubtedly different for different planets, in the sense that the massive ones collected a larger fraction of the proto-planet than did the smaller ones. This may well be the interpretation of the differences in slope presented by equation (5) as compared to equation (4); if this interpretation is correct, it means that the largest planets collected about one hundred times as large a fraction of their proto-planets as did the smallest ones (0.1–0.01 and 0.001–0.0001, respectively). . . . One final remark may be made on the origin of orbital inclinations. According to the developments in section 2, the gaseous disk surrounding the sun flattened until it reached the critical (Roche) density, after which it broke up into a pattern probably set by turbulence. If one started with a gas cloud of very low density, such a cloud would have had to get exceedingly flat before the density reached the critical value. The orbital inclinations are thus a measure of the vertical concentration that was required before the Roche density was reached. In the case of the rings of Saturn, the Roche density was never attained, not even upon condensation of the ring; in this case the flattening process has therefore been carried to the extreme." These statements are still regarded as substantially correct.

The subject was worked out in some detail in a chapter written in 1949 for Hynek's *Astrophysics* (Kuiper, 1951*a*). In this chapter it was shown that turbulence as such could not lead to formation of planets and that therefore the scale of turbulence gave no explanation of Bode's law, as had been assumed; that the condensation of massive planets almost certainly required the prior existence of large, gravitationally stable clouds; that such clouds (proto-planets) would in time indeed develop by gravitational instability, as the solar nebula was shrinking parallel to its axis from cooling by radiation to space; that the densities required for gravitational instability to set in with present orbital dimensions did lead

to proto-planet masses of the right order of magnitude; that the origin of binary stars differed from that of planetary systems merely in requiring a larger mass per unit interval of log r; and that, on the other hand, a mass per unit interval in log r smaller than a critical value would lead to no planets at all but, instead, to very numerous small condensations formed by accretion; that the comets so originated outside proto-Neptune. Bode's law was found to be no true law but merely a measure of the initial radial density distribution of the solar nebula expressed in suitable units. The frequency of planetary systems in the galaxy was estimated by extrapolation of the frequency curve of mass ratios in binary stars and found to be one per 10^2 to 10^3 stars, or some 10^9 systems for the entire galaxy.

The sense of rotation of the planets, which is direct, was attributed to solar tidal friction which, regardless of previous motion, caused the rotation of the proto-planets to be synchronous with the orbital revolution. This followed from the fact that all proto-planets, irrespective of the distance from the sun, were initially close to the limit of tidal instability: near Neptune, at 100 times the distance of Mercury, the solar tidal force per unit length was 10^6 times smaller, but the proto-planet was some 10^2 times larger and the self-gravity 10^4 times smaller; the ratio, self-gravity to tidal force, was therefore the same. Later, as the proto-planet contracted, the rotation could no longer be synchronized and became direct. (This treatment was later criticized by Schatzman (1953), as is discussed below). It was shown that subsequent satellite formation is compatible with this origin of planetary rotational momentum. The regular satellites were attributed to a homologue of the planet-formation process, in accordance with the empirical mass-distance relation found in the 1949 paper. It was shown how the subsequent postulated mass loss of the proto-planet led to a proportional expansion of the satellite system and could lead to shedding of satellites into interplanetary space; how these lost satellites could be recaptured by the proto-planets and lead to irregular satellites; or, if not recaptured, possibly to the Trojan asteroids. These problems of satellite origin and the Trojans were later worked out in greater detail (Kuiper, 1951*c*, 1952*b*, 1953*a*, 1953*d*, 1953*e*), and have been strengthened by beautiful investigations by Rabe (1954) on the Trojans and on the asteroids (Rabe, 1956).

In the *Astrophysics* chapter, in which the sun was assumed to be at its present brightness, the computed accretion rate seemed too low to allow the formation of asteroids by accretion. It seemed plausible, instead, that five or ten parent asteroids had formed from small proto-planets, the smallness being attributed to perturbations by massive proto-Jupiter; and that the remainder had formed from these bodies by collisions. When

it became clear that the solar nebula broke up before the sun was bright (Kuiper, 1951*b*), the computed accretion rate increased; furthermore, closer examination showed that the small proto-planets could probably not have formed as supposed. This part of the *Astrophysics* chapter was therefore revised (Kuiper, 1953*b*). According to the new result the asteroids formed in a gravitationally stable part of the solar nebula, like the comets; since they originated some 10 times closer to the sun and since the critical density varies as r^{-3}, they are some 10^3 times larger than the comets, the accretion rate being proportional to the density.

Two further problems were discussed but left unsolved in the *Astrophysics* chapter: the process by which the proto-planets lost their large excess mass; and the origin of the several large planetary obliquities. Some progress on the second problem was actually made, by showing that solar tidal friction would increase planetary obliquities; but this effect was found to be quantatively inadequate by roughly one order of magnitude.

The first problem, of "how the proto-planets could first have formed and thereupon nearly completely dissolved—with only the present planets surviving" was re-examined later (Kuiper, 1951*b*). This problem has never ceased to fascinate the writer. In tackling it he first thought that rotational instability of the contracting proto-planet might have led to the mass loss, in analogy with the mass and momentum loss suffered by the sun in its process of contraction; but this model led to contradictions; instead, with the use of the roughly-known angular momenta of the original proto-planets and the present planets, the material was found to have left the proto-planets at transverse velocities which were a small fraction of the free circular motion at the equator. *Evaporation* was the only mechanism that could satisfy this requirement. Comparison of the roughly-known energy contents of proto-planets and planets then showed that much energy had to be added to the proto-planets to make this evaporation possible—which only the sun could provide. This proved that *solar radiation was responsible for the near-disintegration of the proto-planets*; and, by the same token, that the proto-planets formed before solar radiation was important, i.e. during the Helmholtz-Kelvin period of solar contraction and that the solar nebula was really formed by the contracting sun itself.

A new problem became apparent however: how were the evaporated gases, largely hydrogen and helium, subsequently removed from interplanetary space? This problem was discussed at the 1952 Astrophysical Conference at Liège (Kuiper, 1953*c*, 1952*b*). It was found that when the sun completed its contraction and became bright, it created an H II region in the more tenuous parts of interplanetary space, whereupon

solar corpuscular radiation swept out the ions by a process first considered by Biermann (1951) in explanation of comet tails of Type I. The denser regions replenished hydrodynamically the vacated spaces, the new material was ionized in turn and swept away. Evaporation at right angles to the plane of the ecliptic accelerated the dispersal. This gradually cleared the entire interplanetary space. Hot exospheres were thereupon established in the outer fringes of the proto-planets and their evaporation to interplanetary space began, with interplanetary space being swept clear continually. It was shown that the entire process would run its course in 10^{8}–10^{9} years if the solar corpuscular stream were of the same order of density and velocity as adopted by Biermann in his study of comet tails. In 1952 it seemed possible that a rather smaller density would have to be assumed and some doubt therefore remained as to the adequacy of the dispersal mechanism. However, since then new information on the density of interplanetary ions has come from photometric and polarization studies of the zodiacal light (Behr and Siedentopf, 1953; Elsässer, 1955), which leads to a density near the earth of about 600 electrons per cm.^{3}; and from the interpretation of radio whistlers (Storey, 1953), which gives about 400 electrons per cm.^{3} at 3 earth-radii. A value of 500 electrons and protons per cm.^{3} near the earth appears therefore well established; and with that, the proposed dispersal mechanism of the excess mass of the proto-planets may be regarded as adequate.

The theoretical analysis in the *Astrophysics* chapter was in part quite tentative and incomplete. The process of gravitational instability was applied to the solar nebula, although Jeans (1902) had derived it for density fluctuations whose dimensions were small compared to those of the medium. This weakness was considered (Kuiper, 1951*a*, p. 396) but the extrapolation seemed to lead to essentially correct results. Theoretical investigations by Ledoux and Chandrasekhar have since clarified the picture. Ledoux (1952) showed that in a flat, isothermal nebula gravitational instability will set in even if the thickness is not large compared to the density fluctuations considered; and that the relevant nebular density is then half the density in the plane of symmetry. Chandrasekhar (1951) showed how turbulence in the nebula may be allowed for, as an addition to the temperature term; and, later (1954, 1955), that rotation does not change the limiting density for the onset of gravitational instability, but only the time scale of its development.

These theoretical results were incorporated in an improved analysis of the break-up of the solar nebula (Kuiper, 1953*d*) in which also the before-mentioned result was used, that the solar nebula broke up *during* the contraction of the sun. Reasons were given why the proto-planets at their maximum dimensions would not, in fact, quite touch in their motion

around the sun, but be smaller. The reduction factor could be determined empirically for proto-earth and proto-Jupiter for which rather reliable initial masses were known independently (480 and 28 times present values); the factor was found to be 0.5 for the earth and 0.7 for Jupiter (revised below to 0.6 with a smaller mass for proto-Jupiter). Furthermore, from the known proto-planet masses and the condition that gravitational instability could set in, an upper limit to the thickness of the solar nebula at the time of break-up could be found; this determined the "quasi-temperature" which included the velocity dispersion in the z-direction due to turbulence. The *quasi-temperature* was found to be 30°–45°K. near the earth and 20°–30°K. near the Jupiter. It was shown that these values were no higher than the real temperatures expected from cooling of the solar nebula during the 10^6–10^7 years available before solar radiation became effective. Accordingly, *the turbulence motions were too small to be so discovered*, or less than the thermal value of some 0.5 km./sec. near Jupiter. It was also shown how turbulent friction, involving loss of energy and conservation of angular momentum, would damp out large-scale turbulence and lead to near-circular orbits in the solar nebula. The lower limit to the mass of a purely gaseous proto-planet was shown to be about 10^{-3} sun; smaller proto-planets, about 10^{-4} sun, as required for Mars and Mercury, required previous formation of condensation products in the plane of symmetry. Results derived on the satellite systems showed that, while the largest satellites (the four Galilean satellites of Jupiter and Titan of Saturn) could have originated from genuine gaseous proto-satellites, the remaining dozen regular satellites were of a hybrid origin, starting with the collection of solid condensed matter before gravitational effects could become important. This has a bearing on the formation of the smallest true planets, Mars and Mercury, and possibly even on that of Venus and the earth.

The related problem of *planetary obliquities* was also treated, and it was made plausible that evaporation of the proto-planet envelopes was the cause of the high planetary obliquities. Only the excess of Uranus beyond 90° could not be so explained (tidal friction on flattened proto-planets can cause obliquities past 90°, but this process may not be adequate). It was further found that the evaporation will have increased the orbital *eccentricities*, by a mechanism that resembles the increase of the eccentricity of the Moon's orbit by tidal friction; it is due to the fact that evaporation is strongest at perihelion.

Review of Criticisms. Before proceeding it seems well to consider first the criticisms that have been published on the ideas and results just described.

Jeffreys (1952), in a largely historical review, raises two points, relative to the existence of turbulence in the solar nebula and to the subsequent existence of proto-planets. He questions the importance of turbulence on the basis of a theorem by Rayleigh. When I wrote my *Astrophysics* chapter there was a widespread belief that turbulence in the solar nebula was far more important than the thermal motions; but my own computations (e.g. pp. 382–3) were made on the assumption that the temperature term alone gave the correct order of magnitude, and this did not lead to contradictions; while later (Kuiper, 1953*d*) definite evidence was found that the turbulence term was unimportant during the stage when the proto-planets were formed. Since the initial state of turbulence is not known and the decay time has not been estimated, my semi-empirical result appears to be the only datum available so far. Turbulence could not have completely decayed or all orbital inclinations and planetary obliquities would now be zero. On the existence of proto-planets which later lost most of their mass, Jeffreys (1952, p. 284) states: "I think that this is definitely impossible". This statement appears to be based, however, on the assumption that such mass loss would have to take place by unspecified effects of gravity or viscosity, whereas it had been shown (Kuiper, 1951*b*) that it took place by evaporation. The removal of the resulting interplanetary gas remained uncertain until a later paper (Kuiper, 1953*c*, 1952*b*). It is emphasized that dispersal to infinity does not require a source of angular momentum, but only of energy. The conclusion (Jeffreys, 1952, p. 285): "a disk theory is promising but the general distribution of mass with distance must always have been roughly what it is now", i.e. the use of Babinet's criterion, offers no explanation of the highly selective composition of the planets.

Schatzman (1953) raises three points, the first one identical with the first point of Jeffreys. In the second and third he objects to the treatment in *Astrophysics* leading to the conclusion that planets owe their direct rotation to solar tidal friction at the early proto-planet state. This treatment was admittedly approximate; but the margin involved is so wide that no refinement seemed necessary. Specifically, Schatzman states that tidally-enforced rotation will lead to heating and that later the shrinking of the proto-planets requires a source of energy. These remarks are correct but the effects are much smaller than suggested by Schatzman and do not affect my conclusions. The state of initial rotation is very difficult to determine and appears to require extensive numerical work using restricted three-body trajectories. I started such work in 1951 but the results obtained, at the expense of much time, were insufficient to properly define the initial state. Schatzman, on a two-body approximation, concludes that initially no rotation existed in a non-rotating frame and

computed the heating of the proto-planet by the subsequent tidally-enforced rotation. If these premises are granted, the following results are found. The radius of proto-Jupiter and proto-earth in terms of the orbital radii are found below to be about 0.17 and 0.076, respectively, giving (in Schatzman's notation) $A < 0.0116$ and < 0.0023; $x > 0.976$ and > 0.995; and $\Delta T < 250°$ in both cases. These inequalities are probably rather strong (they were computed for disks without any central concentration). Now ΔT was computed for *instantaneous* heating; actually, the time scale is probably between 10^3 and 10^6 years, so that the temperature rise, in balance with radiation losses, is found to be entirely insignificant, of the order of 1° or less. By a reversal of the argument, subsequent contraction with initial preservation of synchronized rotation, will require a small supply of energy, which initially can come from internal rearrangement of the proto-planet (core formation) and later also from solar radiation. I should add that when I wrote my paper I was unaware that Poincaré (1913, p. 52) had used the explanation for the direct rotation of planets in almost identical terms, except that he was in doubt about Uranus and Neptune which, on the basis of my work, should be treated as the nearer planets.

Ter Haar (1953) raises two main questions. One is concerned with the best representation of the empirical relation between planet and satellite separations and masses; and the other with the thickness and mass of the solar nebula. On the first there is no disagreement in fact. Neither Bode's law nor the μ/Δ^3 law represents the data very closely as was apparent from my diagram (Kuiper, 1949, p. 311). What is important is that the μ/Δ^3 law was intelligible and indicated that tidal stability determined the initial planet or satellite masses. Once that was clear Bode's law *and* its deviations became intelligible (Kuiper, 1951*a*, p. 393f). The corresponding problem for the satellites was reconsidered later more closely (Kuiper, 1953*a*, 1953*d*). On the second point there is disagreement; ter Haar assumes enormous turbulence velocities and concludes from this that densities suitable for planetary formation (Roche density) require a solar nebula with a mass far in excess of the sun itself. The assumption of turbulence on a large scale has no theoretical foundation; the energy of the turbulence spectrum is an integration constant and may be zero. Clearly, the assumption is unsuited to explain proto-planets. On the question as to what the paper by Chandrasekhar and ter Haar (1950) had proven I had consulted Dr. Chandrasekhar. The footnote on p. 5 (op. cit.) is based on a misunderstanding.

(*To be concluded*)

THERMAL ASPECTS OF THE ORIGIN OF METEORITES

By J. A. Jacobs and D. W. Allan

The origin of meteorites is one of the perennially controversial topics of science. This is not to say that every detail of their history is in doubt; there is little question that they originate in the asteroid belt between Mars and Jupiter. Furthermore, it is generally agreed that they are fragments of a larger body (or bodies) which, before being broken up in some manner, had undergone a heating process which caused differentiation of its material, with the formation of an iron core, and the concentration of radioactivity towards the surface. Thus iron meteorites are considered to have come from the central regions of such a body and chondritic meteorites, which have higher than average radioactivity, from near its surface. What is in dispute is, first of all, the nature of the heating process—did the body begin hot, or was it formed by cold accretion and later melted by its own radioactivity—and, secondly, the size of the parent body—was it a planet, the disruption of which produced the asteroid belt, or are the meteorites and many of the small asteroids the products of collisions amongst bodies of the same size as the present asteroids (about 1000 km. or smaller)?

A similar lack of agreement prevails in the interpretation of the experimental data on meteorites. Brown and Patterson (1948) employed chemical thermodynamics to study the distribution of iron and nickel in meteorites, and deduced that they had originated under conditions of high pressure and temperatures of about 3000°C., such as would be found in a planet. Klotz (1949) and later Urey and Craig (1953) disagreed completely with these views. Uhlig (1954, 1955) summed up metallurgical evidence on the iron meteorites, and concluded that the Widmanstätten pattern of octahedrites formed under high pressure and slow cooling to about 300°C., and that Neumann bands were formed by fragmentation of a parent body which had cooled to less than about 600°C. Vogel and Heumann (1946) had earlier stated that the Widmanstätten pattern formed under low pressures. There is a measure of agreement on the ages of stony meteorites; these are of the order of billions of years, and although one age as low as 2×10^9 years has been reported (Thomson and Mayne, 1955), the work of Wasserburg and Hayden (1955) and of Patterson (1955) suggests an age of over 4×10^9 years for the stones. There is definite disagreement on the ages of the irons. Patterson thinks that the irons are about the same age as the stones, whereas Reasbeck and Mayne (1955), who have continued the helium age determination method developed by Paneth, believe they are

only 2 or 3 $\times$ 10^8 years old. They explain this difference in ages by a planetary origin in which the iron core solidified much later than the surface regions. (See this JOURNAL, November-December, 1955, p. 254–5). Many workers (e.g. Urey, 1955), however, doubt the value of results by the helium method.

The authors have recently completed an extensive study of possible thermal histories for the asteroidal-type bodies from which the meteorites originated (Allan and Jacobs, in press). Their method was to examine the central and average temperatures produced within homogeneous spheres by various initial concentrations of radioactivity. The effect of varying both the size and thermal conductivity of the spheres was investigated. Nine different sets of initial radioactive concentrations were examined. These cover a wide range of possibilities, from about the highest values likely to the low values which would be expected for differentiated material. Two different starting times (i.e. two possible ages for the solar system) of 4.5 and 5.5 billion years were considered. None of the thermal histories thus examined fits completely the evidence discussed above. For example, the central portions of the asteroids could not have cooled slowly to about 300°C., as Uhlig requires, unless the diameter of the asteroids were about 1000 km., and the central pressure would then be an order of magnitude less than he suggests. However, some of the histories are in accord with a large portion of the experimental evidence, and it is possible to draw, at least tentatively, a few general conclusions, which may be summarized as follows:

(1) A hot origin for the asteroids leads to a reasonable thermal history.
(2) An origin by cold accretion and subsequent radioactive heating would be feasible if the time of origin were 5.5 billion years ago, or if the concentrations of radioactivity were higher than is at present thought reasonable.
(3) In both cases, the asteroids from which the meteorites originated were probably about 1000 km. in diameter, in agreement with Kuiper's (1953) theory of the origin of the asteroids.
(4) The pressure and temperature requirements deduced from metallurgical studies may not be compatible, and the helium ages for iron meteorites are hard to reconcile with other data.

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REVIEW OF PUBLICATIONS

On the Origin of the Solar System by H. Alfvén. Pages 194; 6 × 9¼ in. Oxford, The Clarendon Press, 1954. Price $4.50.

Professor Alfvén begins his book with a quotation from a recent paper by Chandrasekhar "It is the usual fate of cosmogonical theories not to survive". As one closes the book one cannot help wondering whether the author has prophesied the fate of his own theory. This is not to belittle the work which contains many valuable ideas on cosmologial processes, but one would like to see some of the steps in his argument developed in more detail.

The author favours a gradual evolutionary process for the formation of the solar system, rather than some cataclysmic action such as a collision with another star or a nova explosion. His theory is thus a modification and development of the early ideas of Kant and Laplace. However, unlike von Weizsäcker and Kuiper, who in recent years have developed these theories on the assumption that only mechanical forces are of importance, Professor Alfvén has developed a theory in which electromagnetic forces play a dominant role. The present book is a modification of the ideas that he published ten years earlier. The revision is largely due to an increased knowledge in cosmical electrodynamics, no small part of which is due to Professor Alfvén himself. Whether electromagnetic forces are negligible or not depends mainly on the degree of ionization of the "initial cloud" from which the solar system is supposed to derive. If the degree of ionization and hence the electrical conductivity was negligible, Kuiper's "cold" theory is the more likely, otherwise electromagnetic forces cannot be ignored.

Professor Alfvén considers the separation by diffusion of the different constituents of the initial gas cloud which is acted upon by the opposing forces of the sun's gravitational attraction and a magnetic field. The gravitational force acts on the non-ionized constituents which are magnetically held. Owing to electromagnetic action the falling gas clouds become ionized and stopped at certain distances which roughly correspond to the present positions of the planets. Angular momentum is transferred from the sun to the gas clouds by electromagnetic forces causing a concentration of gas in the equatorial planes. Then through condensation the gas is transformed into small solid (or liquid) bodies, and the planets were formed by the agglomeration of these bodies. At a late stage in the formation of the major planets, a similar process occured around them leading to the formation of the satellites. It is suggested that all the above processes went on at the same time so that the density of the gas was very low during the whole formation in contrast to the "cold" theories which usually assume that all the matter which now forms a planet was once dispersed as a gas in the region of the present orbit of the planet, leading to a rather high density of this gas. It is essential to Professor Alfvén's theory that the sun should possess a magnetic field with a very high dipole moment. The formation of the satellite systems requires likewise that the mother planets possess magnetic fields.

J. A. Jacobs

The Elements of Astronomy by Edward Arthur Fath. Fifth edition. Pages 369 plus star charts; 6 × 9 in. New York, McGraw-Hill Book Company, Inc., 1955. Price $6.60.

The first edition of *The Elements of Astronomy* appeared in 1926, and since that time this book, in its various editions, has been widely used as a text-book for elementary classes in astronomy. Many new ideas and techniques have been developed since the fourth edition was printed in 1944, so that the present edition contains numerous changes. The whole format has been modernized, with many of the diagrams re-drawn to make the lettering uniform and more easily read. In general the new style is more attractive, although the type used for Roman numerals makes the designation of comets rather hard to read.

As in the earlier editions, the solar system is discussed in detail, and over two-thirds of the book is devoted to it. This leaves little space for the fascinating problems of the stellar system and external galaxies, so that these phases of astronomy are only briefly discussed. However, new

concepts developed since the fourth edition was published have been included.

Readers of this JOURNAL will be interested in the references to the work of Canadian astronomers included in the book. In finding the change of the period of rotation of the sun with latitude only the spectrograph allows determinations at very high latitudes; the values determined by J. S. Plaskett and R. E. DeLury are quoted. Our readers are familiar with the work of P. M. Millman and D. W. R. McKinley in the field of meteors; they will find references to this work in *The Elements of Astronomy*. Our Dominion Astronomer, C. S. Beals, is represented by his analysis of interstellar lines, which proved the existence in some directions of two or more clouds of interstellar material, moving as slightly different speeds. Pioneer work in galactic rotation was done by J. S. Plaskett and J. A. Pearce, who studied the motions of the hot, early-type stars.

R. J. N.

Meteor Astronomy by A. C. B. Lovell. (The International Series of Monographs on Physics). Pages 463; 6¼ × 9½ in. Oxford, the Clarendon Press, 1954. Price $9.00 (Canada).

The direct observation of meteors by radio dates back even earlier than the Leonid shower of 1932 but it was not until 1945, with the facilities provided through the development of radar, that this approach to meteors really began to be exploited. The result has been a transformation.

Professor Lovell was among the first workers in this field after the war. The study of meteors forms an important part of the work at The Jodrell Bank Experimental Station, of which he is director. Recognizing the revolution for what it was, he has set out in this book to tie together the early visual and photographic results and the new information rapidly accumulating. The monograph deals only with the astronomy of meteors; meteor physics, by which is meant heights, upper atmosphere effects, and theories of meteor evaporation, is reserved for a second book.

After a review of the early techniques and two chapters on the radio echo observations, the remainder of the first half of the book is devoted to the sporadic meteors, particularly their velocities. The theme through this section is the controversial question of whether or not the velocities of sporadic meteors exceed the parabolic limit. Are they interstellar objects or members of the solar system? The story makes good reading if one has the ability and patience to follow the details of the various techniques and the arguments and criticisms of the opposing camps, all

of which are ably and fairly set forth. For those who have the habit of reading the last chapter of a mystery thriller first, turn to page 247.

The second part of the book deals with the major meteor showers, "the dominant observational events in meteor astronomy". One of the successes of the radio approach has been the discovery of the day-time showers. The new and the old are brought together in these chapters where, for example, it is shown in several cases that a day-time shower is simply another crossing by the earth of the orbit of a well-known night-time shower. The last chapter is on cosmological relationships of meteors.

It is no fault of Prof. Lovell's that the book was out of date almost before it was finished (early 1952). Rather it underlines the tremendous advances being made in this field from year to year. An appendix summarizes progress up to the end of 1953, but the book was not released until 1955.

Canadian readers will be interested in the part played by the Ottawa meteor astronomers in settling the question of meteor velocities. The new Super-Schmidt cameras, although capable of critically decisive observations on this question, are only briefly mentioned, because, as the author points out, results were not available.

The book succeeds in fulfilling its aim of surveying the field at a critical turning point in its development, of interpreting the new in terms of the old, and of setting the stage for rapid advances in the future.

D. A. M. R.

Astronomy by Robert H. Baker. Sixth edition. Pages viii plus 528, 6¼ × 9¼ in. Toronto, D. Van Nostrand (Canada) Limited, 1955. Price $6.00.

This book is the newest edition of perhaps the most widely used elementary text-book in astronomy. Its wide popularity is an indication of its quality.

The first half of the book deals primarily with our own solar system. The earth and its motion, time, the moon, the solar system, the physical nature of the planets, and the sun are all discussed. The last half of the book discusses the nature of stars, variable stars, binaries, star clusters, interstellar gas and dust, the galactic system, and finally the external galaxies and the universe. This book is particularly valuable as a text-book because many revisions have been made from time to time for the purpose of keeping it modern. In this edition, for instance, is a discussion of stellar evolution which embodies information only two or three years old. There are also sections on radio astronomy, an entirely new and

important branch of astronomy. Also included is an article written by Dr. Gerard P. Kuiper on his theory of the origin of the solar system (see also this JOURNAL, vol. 50, p. 57).

In spite of the many revisions of this book, there are a few ways in which it might be improved. The treatment of Kepler's third law involving masses is written in such a way that it is valid only for particular, and often unsuitable, units. As a result students invariably use the equations incorrectly. The chapter on variable stars is very poorly placed. At present it is located among chapters which deal with masses, radii, and luminosities of stars. It would be much better if it followed the chapter on stellar atmospheres and interiors. Stellar spectra are treated in several chapters; they could be handled almost entirely within a single chapter.

In spite of these minor faults, the book is still one of the best introductions to modern astronomy. One should perhaps also mention the fine illustrations and photographs included; they have been prepared or chosen with great care.

J. B. O.

Tables of the Error Function and Its Derivative; National Bureau of Standards Applied Mathematics Series 41. Pages xi plus 302; 8 × 10½ in. Washington, U.S. Government Printing Office, 1954. Price $3.25 U.S., $4.34 foreign.

This is a reissue of an earlier table in the same NBS Series, with a few misprints corrected. The error function and its derivatives are of widespread application in science. Their values are given here to fifteen decimals over a very wide range and with small tabular intervals. The explanatory text does not favour one particular type of application, thus avoiding a shortcoming of previous tables. This publication will rank as the ultimate standard reference for these important mathematical entities.

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The sine and cosine integrals have long played an important role in the theory of numbers and the calculus of probabilities. In these fields general properties of the integrals are of more interest than actual numerical values. In recent years, however, a need has arisen for

numerical values for these integrals because of interest in such fields as nuclear physics, antenna theory, transmission-line theory, diffraction theory, and acoustics. The table published in this volume contains $\mathrm{Si}(x)$ and $\mathrm{Ci}(x)$ for values of x running from 10 to 100 in steps of 0.01. A few pages of additional useful tables are also given.

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The History of the Telescope by Henry C. King. Pages xvi plus 456, 7¾ × 10 in. Cambridge, Mass., Sky Publishing Corporation, 1955. Price $12.50.

It may well be that in the present decade we are witnessing the end of an era in the history of the telescope. The giant of Palomar, in operation since 1950, may, it is true, someday be surpassed as, in turn, have been the 9½-inch Dorpat refractor of Pulkowa, the 40-inch refractor of Yerkes, the 100-inch Hooker reflector of Mount Wilson. On the other hand, there is a growing likelihood that astronomical instrumentation is about to take another turn—a turn towards the increased efficiency of telescopes of present size by electronic means. And apart from this trend, there is a notable switch of emphasis towards radio telescopes.

For these reasons, now appears to be a most appropriate time for astronomical literature to be enriched by a historical work on telescopes. It is hard to imagine how it could have been done better than in this beautiful book, *The History of the Telescope*, by Dr. H. C. King, Senior Lecturer in the Department of Ophthalmic Optics, Northampton Polytechnic, London. As Sir Harold Spencer Jones writes in his foreward, the book "fills a notable gap in astronomical literature".

The story of the telescope, since its beginnings in the early 1600's, is a fascinating zig-zag of development. The early simple refractors of Galileo and Scheiner suffered so much from chromatic and spherical aberration that they were useless beyond the basic observations described by Galileo. Then began the amazing phase of the long telescopes, increased focal length being the only known cure for the aberrations. In the patient hands of Hevelius, Huygens and Cassini these unmanageable bean-poles, upwards of 200 feet in length, somehow yielded up the secrets of Saturn's rings and the existence of many of the satellites.

Obviously this line of development was a *cul de sac*; indeed, Newton dealt what appeared then to be the death blow to the refractor when he stated that chromatic aberration could never be eliminated in a lens. However, Newton offered the reflector as an answer to the dilemma, suggested the arrangement which now bears his name, and made a small demonstration model with a spherical speculum. Within a few years

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Obviously this line of development was a *cul de sac*; indeed, Newton dealt what appeared then to be the death blow to the refractor when he stated that chromatic aberration could never be eliminated in a lens. However, Newton offered the reflector as an answer to the dilemma, suggested the arrangement which now bears his name, and made a small demonstration model with a spherical speculum. Within a few years

(this was in the 1670's) Gregory and Cassegrain had suggested alternative forms of the reflector and Hadley had succeeded in parabolizing a mirror. One of the great ages of the reflector had begun, an age which saw William Herschel grinding mirrors up to 48 inches in diameter from bell-metal blanks which he cast himself. But again it was a *cul de sac*; the tremendous effort that went into these metal mirrors brought little reward, for they tarnished to a useless state in a few months. The last metal giant, Lord Rosse's 72-inch "Leviathon of Parsonstown", completed in 1848, cost £12,000 and produced only one discovery of note—the structure of a few of the nebulae. Meanwhile there was new hope for refractors springing from the Dollands' invention of the achromatic doublet in the 1750's, and the next 150 years saw refractors forge steadily ahead in quality and size at the hands of Ramsden Fraunhofer, Lassell, De la Rue and the firms of Troughton and Simms, Mertz and Mahler, Thomas Cooke, Carl Zeiss and finally Warner and Swasey who built the two greatest refractors, the Lick 36-inch and the Yerkes 40-inch. Once more the end of the road had been reached, but again there was another road which had opened up with the discovery by von Steinheil and Fraunhofer of the silvering-on-glass process in 1856. The great glass reflectors have now reached their acme in the Palomar 200-inch giant. Indeed, the reflector seems to have won the race, for the Schmidt telescope and its variations, which are more reflectors than refractors (though they are a little of both), have wrested even the field of wide-angle photography from the astrographic refracting telescopes.

This story, with a wealth of detail, has been chronicled in a most fascinating and scholarly manner by Dr. King. He has managed to include the technical details which will be wanted by the scientist without making his narrative so dependent on them as to discourage the non-scientist reader. His book is profusely illustrated with diagrams and with photographs or drawings of nearly every important telescope and telescope maker. Most important of all, he has given complete references to his source material—more than 1600 of them.

The History of the Telescope is a book for everyone who has an interest in telescopes or in astronomy. For the professional astronomer it has all the important facts of the world's great telescopes, the excellent index and the important references to the original papers. For the general reader it has an inspiring story of human progress. For the amateur telescope maker it will be an unending source of ideas. Indeed, Dr. King has a particular respect for the amateur and his contributions, reminding his readers that in every period some of the greatest telescope makers were amateurs in the best sense of that word.

J. F. H.

NOTES FROM OBSERVATORIES

Dominion Observatory, Ottawa

A successful experiment was tried in December when the staff entertained at their first Christmas party to include children. About 30 of the younger generation joined in the fun and received gifts from Santa Claus. . . . On March 2nd J. P. Henderson retired from the staff after thirty-seven years of service. An informal gathering was arranged in his honour and Mr. Henderson was presented with a wrist watch. "J.P." leaves shortly for a European holiday to visit places which he missed on a flying trip last spring. . . . Recent visitors to the Observatory have included Dr. M. C. Gerson of the Air Force Research Centre, Cambridge, Mass., Dr. W. O. Roberts of High Altitude Observatory, Boulder, Colo., Dr. G. J. Odgers of the Dominion Astrophysical Observatory, Mr. A. M. Thompson of the National Standard Laboratory, Chippendale, New South Wales, Australia, and Dr. George Garland of the University of Alberta.

Positional Astronomy. A new Mirror Transit telescope similar to one proposed by Atkinson is being developed to replace the present Meridian Circle. It will employ two fixed horizontal telescopes of 10 inches aperture in a north-south line, with an optically flat mirror, midway between them, mounted like a meridian instrument on a horizontal east-west axis. Contracts have been placed for the construction of the major components. . . . In co-operation with the National Research Council, Defence Research Board and the Dept. of Transport, plans have been formulated to exchange information on time and frequency. It is now possible to make intercomparisons between the Essen ring crystal at the Observatory and three others located in Ottawa. This will form the basis for a Canadian standard of frequency depending on astronomical observations with the Photographic Zenith telescope.

Stellar Physics. Spectra of the same meteor have been obtained from both the Meanook and Newbrook Observatories, the latter spectrogram having the highest dispersion of any known meteor spectrum. Plans have been laid for the construction of meteor spectrographs of considerably higher dispersion and light gathering power for installation at Meanook. . . . Using plates taken with the solar spectrograph in the 10,000 A. region, evidence has been found for a term dependence in the centre-limb shifts of solar spectral lines. . . . A curve-following device has been incorporated in the Moll microdensitometer recording circuit which performs the conversion of photographic density to light intensity automatically and permits intensity tracings to be obtained directly.

Geomagnetism. Five magnetic charts of Canada for the epoch 1955.0 were published and made available for distribution. These maps show lines of equal values for declination, horizontal force, inclination, total force, vertical force and annual change applying to each of these elements. On the completion of final checking, a paper entitled "Record of Observations at the Magnetic Observatories Agincourt and Meanook, 1934–1935" was sent to press. . . . Construction has started on a non-magnetic building to house the astatic magnetometer for investigating remanent magnetism in rocks.

Gravity. Two research projects of the division concern the development of new instruments for the measurement of the acceleration of gravity. One involves the reconstruction and improvement of the Mendenhall pendulum apparatus. The new arrangement provides for the heat control of the vacuum case to minimize the effect of the temperature coefficients of the bronze pendulums, and permits two pendulums to be swung at a time in anti-phase to eliminate the errors due to the sway of the case. This project is in an advanced stage and is nearly ready for field trials. The other project is the development of a gravimeter suitable for the measurement of gravity on the ice of lakes or submerged by cable, or at sea in a submarine. This apparatus, only in the initial stages of development, depends for its operation on the change in the frequency of vibration of a vertical wire stretched by a weight.

Seismology. Three new seismograph stations have been set up, one at Banff, one at Knob Lake and the third at Springhill, Nova Scotia. It is hoped that the installations at Banff and at Knob Lake will become permanent ones if the sites prove satisfactory. The installation at Springhill is a temporary one. It is intended to assist the Mines Branch in the study of rockbursts in the coal mines there.

Personnel. Recent additions to the staff are J. W. M. Geuer of the draughting section and L. G. Hart who is working on the solar research programme. G. R. Small has been transferred to the Meanook Magnetic Observatory to undertake studies in earth-currents and atmospheric electricity. J. P. Henderson attended the meetings of U.S.R.I. in Gainsville, Fla. and visited the U.S. Time Service station at Richmond, Fla. R. G. Madill recently returned from an inspection trip to Agincourt Magnetic Observatory. Dr. C. S. Beals and Dr. Luise Herzberg presented papers at the spring meetings of the A.A.S. in Columbus, Ohio. Dr. R. L. G. Gilbert visited Houston, Texas in connection with Gravity research. Dr. J. H. Hodgson attended meetings of the Seismological Society of America and of its Board of Directors at Reno, Nevada, at the end of March. During his trip he visited seismograph stations at Saint Louis, Berkeley, Seattle, Victoria and Banff. M. S. B.

METEOR NEWS

By Peter M. Millman, *National Research Council, Ottawa*

Meteorite Publications in the U.S.S.R.

A very valuable publication in the field of meteoritics is the Russian publication *Meteoritica*. It is issued under the auspices of the Committee on Meteorites of the Academy of Sciences at Moscow and the first number appeared in 1941. The latest, No. 13, is dated 1955. Since *Meteoritica* is not easily available in many libraries I list below the publication dates of the various numbers:

No. 1–1941	No. 6–1949	No. 11–1954
2–1941	7–1950	12–1955
3–1946	8–1950	13–1955
4–1948	9–1951	
5–1949	10–1952	

Dr. E. L. Krinov, chairman of the Meteorite Committee, writes me that No. 14 is now in press and that No. 15 is in the course of preparation. In addition to very detailed studies of the meteorites themselves *Meteoritica* includes much information concerning the circumstances of fall of these meteorites and an analysis of the observations of the associated fireballs. Most of the numbers have been published without any abstracts in a non-Russian language.

Dr. Krinov also informs me that a comprehensive work on the Sikhote Alin meteorite fall of 1947 is now being prepared. This will appear in three volumes and will give a complete account of the scientific investigation of this unique crater-producing fall.

Orionid Observations in Montreal and Deep River

Orionid observations on Oct. 21/22, 1955, were organized by Miss I. K. Williamson, in Montreal, and by Miss E. A. Taylor, in Deep River. The sky was clear at Montreal throughout the period of observation but clouded up at Deep River at 2345 hrs. A summary of these observations is given below.

1 Montreal (R.A.S.C. Observatory), lat. 45° 30′ 42″ N., long. 73° 34′ 53″ W.

Personnel: W. H. Birtles, E. E. Bridgen, B. Cockhill, F. DeKinder, Miss J. Dickinson, D. E. Douglas, M. Fortier, Miss O. Giroux, C. M. Good, B. Jones, A. Laur, J. Mor-

rison, B. Muhlstock, T. Noseworthy, R. Venor, Miss I. K. Williamson, Mrs. D. Yane, D. Zackon, Mrs. K. Zorgo.

In period 2200–0105 hrs. 50 meteors were plotted.

2 Deep River, lat 46° 06′ N., long. 77° 29′ W.

Personnel: M. A. Clark, D. E. Coates, Miss L. M. F. Corbeil, Miss D. Grieve, J. W. Hilborn, G. Jones, W. T. Sharp, Miss E. A. Taylor.

In period 2305–2345 hrs. 18 meteors were plotted.

The observed hourly rate for all meteors at Deep River was 41 and, since we can assume that six observers were continuously watching the sky, this is the standard rate. It agrees very closely with the Ottawa rate for the same time as published in the last issue of the JOURNAL. In Montreal the rate was considerably lower, 16 per hour, in spite of a larger observing party. The difference undoubtedly is due to the haze and bright sky found at the centre of a large city. The magnitude distributions of all meteors observed are listed in the table:

Magnitude	1	2	3	4	5	Total	Mean Mag.
Montreal	8	17	18	5	2	50	2.52
Deep River	1	8	7	2		18	2.56

Neither of the above groups identified meteors as to shower but an examination of the plots indicates that in each case about one quarter of the meteors recorded were Orionids. The greater part of these observations were carried out before midnight and the percentage of Orionids generally increases in the latter part of the night when the radiant is higher.

VARIABLE STAR NOTES

American Association of Variable Star Observers

By MARGARET W. MAYALL, *Recorder*

UX Cygni 205030. Mr. V. Osvalds, of the Leander McCormick Observatory, has noted a rapid change in brightness of the long period variable UX Cygni (*P.A.S.P.*, vol. 67, p. 417, 1955) on a double-exposure proper-motion plate taken on Oct. 14, 1933 with the 26-inch refractor. The photovisual magnitude of UX Cygni was 10.4 on the first exposure, and 12 minutes later it was 11.8. The star was 11.8 on each of the two exposures on a plate taken on Oct. 4, 1933.

In 1951, I was looking for a good identification plate of UX Cygni in the Harvard Observatory photographic collection, and happened on two plates taken on the night of Aug. 15–16, 1944. The plates were taken with the 1½-inch Cooke patrol camera (scale 600″/mm.) and were very poor for faint stars. The image of UX Cygni was blended with the 11.7 comparison star about 1′ north of the variable, but the combined image appeared to be more than one magnitude brighter on the second plate than on the first. A plate taken on the following night showed the image bright; and one a week earlier was also bright.

	Date	J.D.	Exp.	Mag.
1944	Aug. 8– 9	31311.654	61^m	9.9 ?
1944	Aug. 15–16	31318.650	30^m	11.2 ?
	"	.694	80^m	9.9 ?
1944	Aug. 16–17	31319.713	102^m	9.9 ?

These observations were decidedly too poor to publish at the time, but in view of Mr. Osvalds' announcement, they may help confirm the rapid variations of UX Cygni, and give an added incentive to secure more accurate observations in the 1956 season.

The observed maximum of UX Cygni in 1944 was on J.D. 2431340 (Campbell, *Studies of Long Period Variables*, page 102, 1955). The predicted maximum for 1956 is on July 3 (*A.A.V.S.O. Bulletin* No. 11, page 6, 1956). The period of UX Cygni is 560 days, and the rapid irregularities seem to occur just before maximum.

The following report on the variation of Zinner 357 was presented by Mr. Edward Oravec at the 1955 Annual Meeting of the A.A.V.S.O. Preliminary work on the study of the light variation was made possible under a grant received by the A.A.V.S.O. in 1952 from the Office of Naval Research for the "Study of Bright Semi-regular and Irregular Red Variable Stars."

ZINNER 357

By EDWARD G. ORAVEC

For a long time, the star known as "Zinner 357" or "E-B 120" has been thought to be a variable star. This star was first listed by Birmingham (numbered "BII 16 Lep") in his *Catalogue of Red Stars* published in 1877, and it was listed as a variable or suspected variable in numerous other books on astronomy in the late 19th Century.

In Espin's edition of Birmingham's *Catalogue of Red Stars* published in 1888, it was listed as "E-B 120." Other references to this star were made in Webb's *Celestial Objects for Common Telescopes*: in the 4th edition (1881) as a 5th magnitude variable, "very fine," according to Morton; in the 6th edition (1918) it is listed as "Variable? 6.5 mag." In Chambers' 3-volume set *Astronomy*, Volume III, 1890 edition, a note is made for No. 19 in his Catalogue of Suspected Variable Stars, probable range of magnitude 5½ to 7½, authority: Burton, "very red star." Espin also thought it was certainly variable.

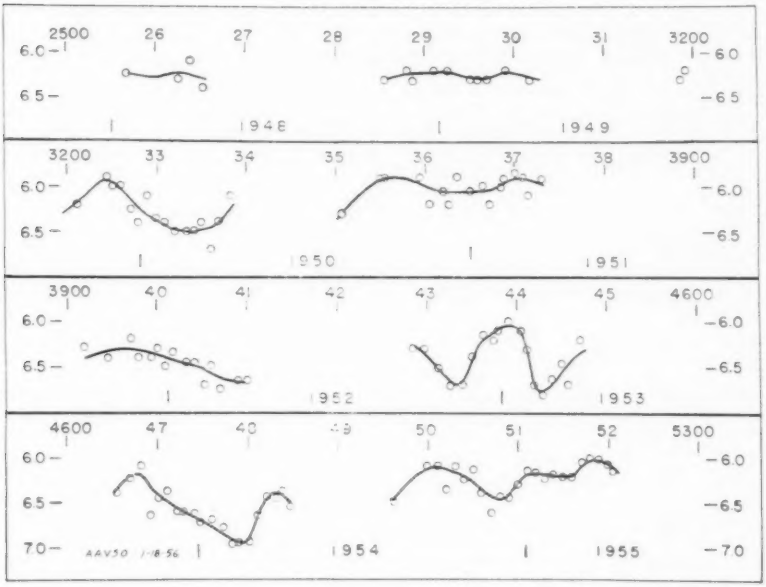
In 1948, Mr. Gilbert Matthews and I, both members of the A.A.V.S.O., noticed this suspected variable in one of the mentioned lists and thought it worthwhile to add to our observing programme. The star is located just west of iota Leporis and is shown on the Norton and Skalnate Pleso atlases, but is not numbered. A sequence of comparison stars was made and we commenced observations. In 1955 an A.A.V.S.O. chart of the "a" type (scale 1 mm. equals 5′) was made for W Orionis 050001 and E-B 120 Leporis 050611, showing the magnitude sequence we adopted.

The light curve shown is the 10-day mean of nearly 500 observations from 1948 to 1955. Each circle is for a 10-day period and a line is drawn showing the probable

TABLE I

10-Day Mean Values for Zinner 357

J.D.	Mag.	No.	J.D.	Mag.	No.	J.D.	Mag.	No.	J.D.	Mag.	No.
2432567	6.25	2	2433555	5.9	1	2434339	6.7	1	2434961	6.5	1
2623	6.3	1	3593	5.9	1	4349	6.40	2	4998	6.10	2
2635	6.1	1	3604	6.2	1	4362	6.15	2	5010	6.10	6
2652	6.4	1	3620	6.05	2	4373	6.2	1	5020	6.37	3
2854	6.3	1	3625	6.2	1	4379	6.1	1	5030	6.10	6
2880	6.20	3	3635	5.9	1	4391	6.0	1	5040	6.25	4
2886	6.3	1	3649	6.05	2	4404	6.1	1	5049	6.24	9
2910	6.2	1	3663	6.0	1	4412	6.30	2	5059	6.39	8
2924	6.2	1	3672	6.2	1	4419	6.70	3	5071	6.62	6
2950	6.3	1	3683	6.0	1	4428	6.80	2	5080	6.43	12
2958	6.3	1	3686	5.9	1	4439	6.63	3	5090	6.45	8
2968	6.30	2	3700	5.85	2	4450	6.47	3	5100	6.30	15
2989	6.2	1	3708	5.9	1	4457	6.7	1	5111	6.14	8
3017	6.3	1	3715	6.1	1	4470	6.20	2	5120	6.16	18
3184	6.3	1	3731	5.9	1	4654	6.40	2	5130	6.23	12
3190	6.2	1	3919	6.3	1	4668	6.25	2	5139	6.19	15
3212	6.2	1	3945	6.4	1	4681	6.10	2	5150	6.22	18
3243	5.9	1	3969	6.2	1	4692	6.65	2	5160	6.22	13
3250	6.0	1	3978	6.40	2	4698	6.47	3	5171	6.04	12
3258	6.0	1	3994	6.4	1	4709	6.38	9	5180	6.00	18
3270	6.25	2	4000	6.3	1	4721	6.62	7	5188	6.02	10
3279	6.4	1	4008	6.5	1	4727	6.62	4	5198	6.07	12
3290	6.1	1	4017	6.35	2	4739	6.63	12	5207	6.15	4
3298	6.35	2	4032	6.45	2	4747	6.73	6	5344	6.2	1
3309	6.4	1	4040	6.47	3	4758	6.70	2	5347	6.20	4
3320	6.5	1	4052	6.7	1	4772	6.79	8	5361	6.20	4
3332	6.5	1	4058	6.50	2	4781	6.96	10	5370	6.30	3
3340	6.5	1	4068	6.75	2	4789	6.95	8	5376	6.30	2
3349	6.40	2	4088	6.65	2	4800	6.95	13	5390	6.25	4
3360	6.7	1	4100	6.65	2	4809	6.66	11	5402	6.40	4
3368	6.40	2	4283	6.3	1	4820	6.45	8	5410	6.55	2
3381	6.1	1	4297	6.3	1	4830	6.45	4	5420	6.60	5
3506	6.3	1	4310	6.5	1	4837	6.38	6	5428	6.70	3
3548	5.9	1	4325	6.7	1	4845	6.55	2			



050611 Zinner 357. Mean Light Curve.

variation. The range appears to be from 5.9 to 7.0, although greater extremes have been observed. No period can be derived at present. The star probably is a typical red irregular type. The greatest variations were observed in the 1952/53 and 1953/54 periods. Most of the observations from 1948 to late 1953 were mine; and since then the "Observers Group" of the Amateur Astronomers Association in New York has been following the star.

The star is H.D. 33664, 1900 position $5^h\ 6^m\ 43^s$, $-11°\ 58'.4$, spectrum gM6. It is also No. 110456 in the *Catalogue of Suspected Variables* by Kukarkin and Parenago, 1951.

Nova Search Reports (from George Diedrich, Division Chairman).

Listed below are the "area-nights" of Nova Search done by our observers. The name is followed by the number of area-nights observed in November and December 1955, and in January and February 1956. In the case of Earl Milton the four figures are for area-nights observed in September, October, November and December 1955, respectively.

Richard Bates, 0, 0, 6, 0; Jim Breckinridge, 0, 0, 2, 9; DeLorne Diedrich, 1, 1, 2, 3; George Diedrich, 7, 5, 3, 12; Earl Milton, 12, 16, 22, 16; Beaufort Ragland, 6, 8, 5, 0; Louis Rick, 3, 3, 6, 14.

A.A.V.S.O. observations received November-December 1955, and January-February 1956: November, 4,186 from 61 observers; December, 4,370 from 47; January, 3,753 from 49; February, 3,588 from 55, a total of 15,897 for the four months.

Observer	November–1955–December				January–1956–February *			
	No. Var.	No. Ests.	No. Var.	No. Ests.	No. Var.	No. Ests.	No. Var.	No. Ests.
Adams, R. M.	55	126	60	156	59	159	60	137
Ancarani, M.	19	25	...	...	...	...	...	...
Anderson, C. E.	100	166	37	52	30	53	30	50
Archer, S.	...	...	29	79	...	...	...	...
Aronowitz, C.	164	164	148	190	66	77	91	146
Beidler, H. B.	25	25	...	...	...	...	...	...
Berg, Ray	8	13	13	33	24	55	12	19
Bicknell, R. H.	...	...	9	32	...	...	9	37
Bostic, M.	...	...	...	...	...	...	1	1
Boutell, H. G.	6	6	6	16	...	...	...	...
Breckinridge, J.	19	36	9	13	7	14	3	3
Buckstaff, R. N.	...	...	7	7	...	...	6	8
Carlisle, J. H.	2	3	1	4	1	4	2	5
Carpenter, C. B.	...	...	12	12	...	...	...	...
Cleary, Carl	8	16	...	...	5	5	11	25
Cragg, T. A.	309	332	240	280	80	83	134	137
Crouse, Linda	...	...	...	...	1	1	...	...
Dafter, Mrs. R.	6	38	...	...	...	...	...	...
Dalton, A.	4	4	...	...	...	...	...	...
Darnell, P. B.	2	11	...	...	...	...	...	...
*Darsenius, G. O.	30	144	29	172	21	66	19	60
Deemer, R.	2	2	6	6	22	35	4	7
Diedrich, DeL.	1	1	1	1	1	1	1	2
Diedrich, G.	6	9	7	7	6	7	3	4
Erpenstein, O. M.	10	34	6	16	5	9	8	19
Estremadoyro, V. A.	...	...	...	...	...	...	5	5
Fernald, C. F.	212	319	95	115	181	264	52	79
Ficonetti, R.	23	31	...	...	6	14	...	...
Ford, C. B.	76	82	131	133	156	159	109	111
Gear, M.	2	4	...	...	...	...	...	...
Glenn, W. H.	10	12	...	...	8	19	14	30
Goldenblatt, P. P.	53	80	57	83	95	144	60	141
Greenley, R. M.	3	3	...	...	...	...	...	...
Halvorson, D. O.	15	15	2	2	...	...	...	...
Hartmann, F.	152	162	143	161	148	149	138	145
Haslop, N.	5	7	...	...	...	...	1	1
Hein, G.	61	74	22	22	33	51	39	78
Howarth, M.	20	20	...	...	...	...	18	18
Johnsson, R. G.	...	...	1	1	...	...	...	...
Kelly, F. J.	10	10	...	...	10	10	10	10
Kimball, M.	24	24	22	23	26	26	8	8
de Kock, R. P.	132	411	137	569	118	335	124	450
Kofeod, R.	2	4	...	...	...	...	4	8
Lacchini, G. B.	38	51	...	...	8	16	13	28
Loehde, F.	4	4	...	...	...	...	2	2
Lovi, G.	...	...	4	6	7	10	...	...
Maran, S. P.	56	69	...	...	30	32	33	33
McPherson, C. A.	43	43	...	...	...	...	19	19
Mebius, W. G.	...	...	7	13	4	9	...	...

*Also 91^m DO Cep; $2^h\ 10^m$ UV Cet; 32^m V371 Ori; 50^m AD Leo; $3^h\ 28^m$ EV Lac

Observer	November–1955–December				January–1956–February *			
	No. Var.	No. Ests.	No. Var.	No. Ests.	No. Var.	No. Ests.	No. Var.	No. Ests.
Melville, E. C.	6	6	9	10	12	14	20	28
Miller, R. W.	...	...	18	18	8	8	24	33
Miller, W. A.	5	5	10	10	10	10	6	6
Moloney, K.	...	...	10	32	...	...	...	...
Montague, A. C.	30	45	12	12	21	28	31	45
Morgan, F. P.	11	20	16	27	17	32	18	39
Oravec, E. G.	166	430	218	679	186	461	208	516
Parker, P. O.	...	...	...	...	33	34	30	30
Pearcy, R. E.	8	8	7	7	1	1	3	3
Peltier, L. C.	28	98	26	89	19	80	18	72
Perley, Mrs. R.	5	5	...	...	...	...	...	...
Peters, J. G.	...	...	...	...	1	1	...	...
Pilcher, F.	26	44	...	...	16	16	9	14
Renner, C. J.	...	...	172	209	...	...	...	...
Rizzo, P. V.	18	26	19	21	17	24	18	33
Rosebrugh, D. W.	17	99	16	85	15	72	19	74
Royer, R.	23	31	20	27	21	28	18	23
Schultz, G. W.	7	10	12	15	22	23	5	5
Segers, C. L.	13	38	...	...	22	41	8	14
Skaritka, P.	256	488	232	468	210	506	166	397
Solomon, A. M.	...	...	4	4	...	...	...	...
Solomon, L.	25	25	80	99	43	63	36	49
Taboada, D.	150	160	179	256	222	479	159	343
Thomas, M. A.	...	...	...	...	...	...	4	9
Walko, K.	3	3	...	...	...	...	...	...
Walsh, J. F.	...	...	...	...	1	1	...	...
Weaverling, W.	6	6	...	...	...	...	8	12
Weitzenhoffer, K.	8	8	...	...	...	...	...	...
Wurgel, R. A.	...	...	...	...	...	...	3	3
Wyckoff, J.	18	18	...	...	...	...	13	14
Yamada, T.	23	33	37	98	...	...	...	...
Zahner, D.	...	...	...	...	16	24	...	...

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NOTES AND QUERIES

Some Statistical Relations in Spectroscopic Binaries

In the *Russian Astronomical Journal*, vol. 32, p. 518, 1955, I. A. Daube published his statistical investigation on spectroscopic binaries. He found a definite correlation of some physical elements of these binaries with the spectral type and their position in the space. Such correlations had been suspected by earlier investigators, but the present study by Daube with more reliable material may prove to be more conclusive.

There is a high concentration of B-type binaries toward the galactic plane while a similar concentration of A- and F-type stars fails to show up. In order to find whether there is a preferential plane of the orbits of spectroscopic and eclipsing binaries, Daube computed the ratio $E/(E + S)$ as a function of galactic latitude (E stands for eclipsing, S for spectroscopic binaries). He finds—contrary to some other authors—practically no variation of this ratio with galactic latitude.

Other characteristics of spectroscopic (and eclipsing) binaries have been collected by the present writer in the following table:

Sp. type	$\bar{M}$	$\frac{\bar{R}_1 + \bar{R}_2}{\bar{a}}$	$\bar{G}$
O – B2	34	0.26	115940
B3– B7	15	0.25	23672
B8– A5	4.0	0.14	2438
A6– F5	3.5	0.11	2383
dF6– dG5	2.1	0.09	2364
dG6– dK5	1.7	0.09	692
sgG0–sgK0	4.5	0.14	
gF6– gK5	5.1	0.04	

In this table d refers to dwarfs, sg to subgiants and g to giants. $\bar{M}$ is the mean mass, $(\bar{R}_1 + \bar{R}_2)/\bar{a}$ the relative dimensions, and $\bar{G}$ the mean moment of inertia of these systems.

The following general conclusion can be drawn based on this investigation: Single as well as multiple stars of the same spectral type do not indicate a dissimilar origin. Spectroscopic binaries clearly show the inhomogeneity of our galactic system and in this way they are typical members of different subsystems forming the galaxy. This is especially true of spectroscopic binaries of O-B type. Consequently, they must be

fairly young stars as are the stars in O-associations where they are also found. Another argument in favour of them being young stars is the fact that they are the most massive ones and that they show a high concentration toward the galactic plane. The evolution of spectroscopic binaries must be closely related to the evolution of single stars in the sense that they follow a path along the main sequence from spectral type O to G. This path is indicated by the diminishing mass and moment of inertia of spectroscopic binaries of later spectral type. Finally, the same evolutionary track is shown by the "closeness" of a system, the closest stars being the most massive ones, as indicated by the ratio $(R_1 + R_2)/a$. Both the mass and the radius diminish in the course of evolution but, apparently, the radius does not change proportionally with the change of mass. Consequently the closer systems are associated with the earlier spectral types.

G. A. B.

New Moons

What are the possibilities of finding new moons in the solar system? Not very likely, but according to R. S. Richardson of the Mount Wilson and Palomar Observatories (*A.S.P. Leaflet*, no. 316) there is still some chance. It would be particularly useful to discover moons around Mercury, Venus or Pluto. These planets have no known satellites, and consequently their masses can be determined only from their disturbing effects on other bodies. This requires very long series of painstaking observations. In contrast, once a satellite's period of revolution and distance from its primary are found, a few minutes' computation using Kepler's third law, yields the mass of the planet with considerable accuracy. As well as giving the over-all mass of the planet, the motion of a satellite may give information about the distribution of mass inside the planet.

The mass of Mercury has been found from its effect on the motion of Encke's comet, when it was close about 1842. This value is 20 per cent. less than another value determined in 1950 from observations of the asteroid Eros.

From the disturbing effect of Venus on the motion of Mercury over the interval 1765 to 1937, G. M. Clemence arrived at a value for the mass of Venus of 1/409,000 that of the sun. Other workers, analysing the effect of Venus on the motion of the earth, found a mass of 1/407,000 that of the sun.

The mass of Pluto is the least reliable. Its effect on the motions of Uranus and Neptune is small. The mass is estimated to be about equal

to that of the earth. However, combining this with the diameter of 3680 miles, measured with the 200-inch telescope in 1948, leads to the unusually high value of the density of Pluto of ten times that of the earth. In order to improve the mass of Pluto, observations taken over another century will be needed, unless a satellite is discovered.

The difficulty in searching for a satellite of either Mercury or Venus is the proximity of each of these planets to the sun. In fact it seems that it would be virtually impossible to detect a satellite of Mercury. In the case of Venus, the best time to search would be when the planet is brightest, either before sunrise or after sunset. The planet is then in a crescent phase; a satellite would show the same phase. A satellite would be easiest to detect when at its elongation. The chances of this occurring during the short period available for observation are rather slight.

To detect a satellite of Pluto would require the 200-inch telescope. In order for the image of a satellite to be separated from that of Pluto, and thus detected, the satellite would have to be about 100,000 miles from the planet.

Although the difficulties in the way of detecting other satellites seem overpowering, it is encouraging that as recently at 1948 and 1949 G. P. Kuiper discovered a close satellite of Uranus and a distant one of Neptune.

R. J. N.

PROF. W. M. H. GREAVES, F.R.S., 1897–1955

William Michael Herbert Greaves, the son of Dr. E. C. Greaves, a graduate of the Edinburgh University Medical School, and of Mrs. Greaves, was born in Barbados, British West Indies in 1897, and was educated at Codrington College in the West Indies and at St. John's College, Cambridge. In the course of his brilliant career at Cambridge, during which he was a Wrangler in the Mathematical Tripos of 1919, Smith's Prizeman in 1921, and Isaac Newton Student in the University of Cambridge in 1922, he published a number of papers in the field of dynamical astronomy which attracted immediate attention.

In 1924 Dr. H. (now Sir Harold) Spencer Jones left the Royal Observatory at Greenwich to become His Majesty's Astronomer at the Cape, and the then Astronomer Royal, Sir Frank Dyson, invited Greaves to fill the post of Chief Assistant rendered vacant at Greenwich. While at Greenwich he threw himself with enthusiasm into the observational work of the observatory and, in collaboration with C. R. Davidson and E. Martin, he led the first successful attack on the problem of determining the colour-temperatures of the stars. This work involved the most precise

measurements of the distribution of energy within the continuous spectrum of stars of different spectral types and it has since become a classic of its kind. His interests also turned to the study of terrestrial magnetism and, in collaboration with H. W. Newton, he analysed the Greenwich magnetic storm records covering a period of 53 years.

With the resignation of Prof. R. A. Sampson in 1938, Greaves was appointed Astronomer Royal for Scotland and Professor of Astronomy in the University of Edinburgh, which posts he held along with the Directorship of the Royal Observatory at Blackford Hill until his death on December 24, 1955. He was the Sixth Astronomer Royal for Scotland, following in the line of Henderson, Piazzi Smyth, Copeland, Dyson and Sampson.

Shortly after his arrival in Edinburgh war was declared and Prof. Greaves was concerned with organising an alternative time-service for the country and other forms of war work. Subsequently, he undertook with characteristic energy the reorganisation of the purely astronomical side of the observatory's work. Many important papers have been published from the observatory in the past ten years. Of special note were Greaves' studies of the line spectra of early-type stars—the last to appear was in 1955, in collaboration with E. A. Baker and R. Wilson.

Greaves was elected a Fellow of the Royal Society in 1943. In the years 1948–49 he served as President of the Royal Astronomical Society. He was a Vice-President of the Royal Society of Edinburgh. In the years 1948–55 he served as President of Commission 25 (I.A.U.) on Stellar Photometry.

In 1926 Prof. Greaves married Caroline Grace, daughter of H. D. Kitto, of Melrose, Whiteshill in Gloucestershire, who survives him together with an only son, George.

M. A. Ellison

MEETINGS OF THE SOCIETY

New Honorary Members Announced at the Annual At-Home

At the At-Home on February 17, 1956, the President, Dr. R. M. Petrie announced the election by the Council of three new Honorary Members to our Society as follows:

Prof. Bertil Lindblad, Director of the Stockholm Observatory, Saltsjöbaden, Sweden, past president of the International Astronomical Union, an astronomer highly regarded for his contributions in many fields, particularly in the field of galactic research.

Prof. Ira S. Bowen, Director of the Mount Wilson and Palomar Observatories, Pasadena, California, an astronomer particularly well-known for his work on the interpretation of astronomical spectra and to whom has fallen the responsibility for the organization of the new Palomar Observatory.

Prof. Albrecht Unsöld, Director of the Institute for Theoretical Physics and of the Observatory at Kiel, Germany, an astrophysicist who has made major contributions in the field of stellar atmospheres.

The present list of living Honorary Members is as follows: Dr. Walter S. Adams, U.S.A.; Prof. H. N. Russell, U.S.A.; Prof. Ejnar Hertzsprung, Denmark; Sir Harold Spencer Jones, England; Prof. Harlow Shapley, U.S.A.; Prof. H. H. Plaskett, England; Prof. Otto Struve, U.S.A.; Dr. Walter Baade, U.S.A.; Prof. F. J. M. Stratton, England; Prof. J. H. Oort, Netherlands; Prof. B. Lindblad, Sweden; Prof. I. S. Bowen, U.S.A.; Prof. A. Unsöld, Germany.

AT TORONTO

February 8, 1955—The meeting was held in the McLennan Physics Laboratory. The President, Mr. L. H. Clark, was in the chair. Dr. J. F. Heard extended on behalf of the Hamilton Centre, an invitation to visit the Planetarium at McMaster University March 8.

The meeting was turned over to Dr. H. L. Welsh, First Vice-President, who introduced Dr. A. H. Hollis-Hallet and Dr. G. D. Scott. These three physicists outlined briefly the work of their respective laboratories and invited the members present to visit and inspect these laboratories.

Dr. G. H. Scott, in the Electron Microscopy Laboratory, described briefly the history and development of the electron microscope. The method of preparing specimens for use in the instrument was explained and the RCA EMU2A instrument demonstrated. A fine group of enlarged photographs showing the range and versatility of these powerful scientific tools were conveniently displayed on rotating panels.

Dr. A. H. Hollis-Hallet, in the Cryogenic Laboratory, described the method of cooling helium to within a fraction of a degree of absolute zero. The properties of the gas change at 2°K. becoming super-fluid and super-conductive. Viscosity measures by different methods yield results ranging from 0 to 1/10 that of the viscosity of gaseous air. An induced current will flow in liquid helium for days without detectable decrease in strength, indicating super-conductivity.

Dr. H. L. Welsh explained the operation of the "Light Furnace" used in the study of the spectra of light scattered by gases (Raman spectroscopy). The hydraulic device for producing pressures up to 5000 atmospheres was next described and led to an outline of the studies of spectra produced by gases under high pressure.

RAYMOND R. BROADFOOT, *Recorder.*

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